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Impact of the Program to Support Employment II

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Active Labor Market Policies in Bolivia: Impact of the Program to Support Employment II [*]

Nicolás Campos [†] Manuel Urquidi [‡]

Abstract

Active labor market policies are a set of public policy instruments that seek to promote labor market integration, especially for groups with low levels of employability and income. Evidence indicates that these policies have been effective in promoting access to quality jobs in Latin America and the Caribbean (Urzúa & Puentes, 2010; Card et al., 2010, 2018; Escudero, 2018; Escudero et al., 2019; Carranza & Mckenzie, 2023). The Program to Support Employment II (PAE II), implemented by the Plurinational Employment Service of Bolivia (SPEBO) under the Ministry of Labor, Employment and Social Welfare (MTEPS) and supported by the Inter-American Development Bank (IDB) since 2010 through a loan contract, is part of this type of policy. PAE I was an earlier version of the program that was approved in 2010 and implemented between 2011 and 2018, while PAE II was approved in 2016 and implemented between 2018 and 2022. The general objective of the Program to Support Employment II was to improve the labor market insertion of job seekers accessing the Bolivian Public Employment Service in formal economic units. The specific objectives were: (i) to strengthen the positioning of the Bolivian Public Employment Service; and (ii) to improve the effectiveness of service delivery to job seekers accessing the Bolivian Public Employment Service.

We analyzed the impact of PAE II on employment, employment in formal economic units, and monthly income in Bolivia. We opted for a quasi-experimental approach due to the non-random nature of the assignment of participants to the program: access to the program is universal, and the selection of candidates from the lists prepared by the SPEBO is discretionary on the part of the firms. To identify the causal impact of the program, we combine survey data from beneficiaries and non-beneficiaries, with an empirical strategy that resorts to extensions of difference-in-differences models. The reason for using this estimation strategy is that, in order to see if the program really has an impact on income and employment, we cannot simply compare the income or employment of those who participated in PAE II with those who did not, as there may be many other differences between these two groups that affect the results. We need a counterfactual: an estimate of the employment history that would have been followed by PAE II participants had they not participated in the program. The difference-in-differences model, under certain assumptions, allows us to estimate the counterfactual, and thus to identify the causal impact that the program had on those who participated in it.

We find that participation in PAE II—which offers labor intermediation, direct subsidy, and on-site training for three months—increases the probability of obtaining a job by 14 pp, with this effect being stronger in women (14.8 pp) compared to men (14 pp). In terms of employment in formal economic units, the employment rate increases by 14.9 pp, being higher in women (14.9 pp) than in men (14.3 pp). The monthly income of active beneficiaries increases by 9.5%. This effect is greater in women, with an increase in income of 11.7% compared to 8.1% in men. The impact of PAE II is particularly strong in the short term and decreases over time; however, it has a positive and significant impact on both employment and income up to 24 months after starting the program.

The results indicate that PAE II has effectively reduced barriers to access to the formal labor market in Bolivia and has improved the income trajectory of beneficiaries. This is manifested in an increase in the probability of finding employment in formal economic units and an improvement in monthly income, suggesting an increase in the productivity of active beneficiaries. This increase in productivity aligns with the in-plant training component of the program. These findings are consistent with previous evaluations of the program that also suggest positive impacts on beneficiaries, especially on employment (Novella & Valencia, 2022). Our findings align with the literature indicating that well-designed, targeted, and incentivized training and labor intermediation programs tend to generate sustainable improvements in labor outcomes, especially for groups traditionally facing restricted access to the labor market.

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1 Introduction

The high level of informality is one of the main problems in the Bolivian labor market. Between 2010 and 2021, the average proportion of workers in an informal situation in Bolivia was 80%, one of the highest in Latin America and the Caribbean.[1] The literature suggests that part of the problem lies in the lack of information on job vacancies, which limits the matching between employees and employers, the lack of skills demanded by the private sector, and the lack of work experience.[2] These factors limit workers' access to quality jobs and reduce hiring by companies, affecting primarily young people, Indigenous communities, migrants, women, and people with disabilities.

Active Labor Market Policies (ALMP) address these issues by offering targeted subsidies, training programs, and job placement services. These policies have proven effective in fostering access to quality jobs for vulnerable groups (Card et al., 2010; Kluve & Rani, 2016; Card et al., 2018; Escudero, 2018; Escudero et al., 2019; Carranza & Mckenzie, 2023). The Program to Support Employment II (PAE II), implemented by the Plurinational Employment Service (SPE) under the Ministry of Labor, Employment, and Social Welfare (MTEPS) and supported by the Inter-American Development Bank (IDB) since 2010 through a loan agreement, is part of this type of employment promotion policy. PAE II aimed to improve job placement for seekers accessing the Bolivian Public Employment Service into formal economic units. Its specific objectives were: (i) to strengthen the positioning of the Public Employment Service, and (ii) to enhance the effectiveness of service delivery to job seekers. PAE I, the program's earlier version, was approved in 2010 and executed between 2011 and 2018, while PAE II was approved in 2016 and executed between 2018 and 2022.

In this document, we measure the impact of the Program to Support Employment II (PAE II) on employment, employment in formal economic units, and income. We opted for a quasi-experimental approach due to the non-random nature of participant allocation to the program: access to the program is universal, and the selection of candidates from the lists prepared by the Public Employment Service (SPE) is discretionary on the part of companies. To identify the causal impact of the program, we combined data from surveys of beneficiaries and non-beneficiaries with an empirical strategy that relies on extensions of difference-in-differences models and synthetic control. The treatment variable is defined as having been a beneficiary of PAE II, which involves receiving job placement services, a stipend, and on-the-job training for three months. The main results come from the conventional difference-in-differences estimator and the estimator from Callaway & Sant'Anna (2021). The latter provides an unbiased estimator that is robust to treatment effect

[1] According to the Labor Market and Social Security Information System (SIMS) of the Inter-American Development Bank (IDB), the percentage of informal workers between 2020 and 2021 was: Peru (79.5%), Paraguay (77.5%), Colombia (63.1%), Ecuador (58.9%), Argentina (49.2%), Brazil (36.5%), Chile (28.6%), and Uruguay (23.6%).

[2] Indeed, 25% of those responsible for responding to business surveys indicate that the greatest difficulty when hiring someone new is the lack of sufficient experience (Urquidi et al., 2023).

heterogeneity between groups or over time and is suitable for staggered treatment contexts. We also provide a set of alternative estimators to check the robustness of the results. Specifically, we relax the parallel trends assumption by calculating the program's impact using the synthetic control difference-in-differences estimator from Arkhangelsky et al. (2021). The results are presented along with their respective event studies whenever applicable.

The results show that PAE II has significantly increased the probability of obtaining employment, with a rise of 14.6 percentage points (pp). Similarly, the program increased the probability of getting a job in formal economic units by 14.9 pp. In terms of income, beneficiaries experienced an average increase in their monthly income of 128 Bolivianos (equivalent to 18 USD), which represents a 9.5% increase. The program's impact is more pronounced in the short term and tends to diminish over time, although it remains relatively stable and positive up to 24 months after its inception, both for employment and income. The program had a greater impact on women than men: in employment, 14.8 pp for women versus 14.0 pp for men; in formal employment, similarly 14.9 pp for women and 14.4 pp for men; and in income, greater for women (11.7% and 155 Bs. (22 USD)) compared to men (8.1% and 111 Bs. (16 USD)).

The results indicate that PAE II has effectively reduced barriers to accessing the formal labor market in Bolivia. This is evidenced by an increase in the probability of obtaining employment in formal economic units and an improvement in monthly income. These findings are consistent with previous evaluations of the program (Durand, 2018; Novella & Valencia, 2022), which also suggest positive impacts on beneficiaries, especially in employment. Moreover, our findings align with the literature indicating that well-designed, targeted job training and placement programs with appropriate incentives tend to generate sustainable improvements in labor outcomes, particularly for groups that have traditionally had restricted access to the labor market (Urzúa & Puentes, 2010; Carranza & Mckenzie, 2023).

The document is organized as follows: Section 2 explains the program and institutional context. Section 3 details the data sources and describes the treatment and outcome variables. In Section 4, we explain the methodology used to measure the program's impact. The presentation of the results is in Section 5, and robustness analysis is covered in Section 6. Section 7 discusses the results and concludes. Two appendices follow, including additional figures and tables.

2 Institutional Context: The Program to Support Employment II

In Bolivia, one of the main ALMPs is the Program to Support Employment II (PAE). This service, linked to the Plurinational Employment Service of Bolivia and subordinated to the Ministry of Labor, Employment, and Social Welfare (MTEPS), has been supported by the Inter-American Development Bank since 2010 through a loan agreement. Following the positive results of the first version of the program, a second loan was approved for PAE II. While maintaining its initial design, PAE II incorporated three pilot lines to provide additional support to vulnerable populations: women in non-traditional sectors, people with disabilities, and youth. PAE I was executed between 2011 and 2018, and PAE II between 2018 and 2022. The general objective of PAE II was to improve the job placement of job seekers who accessed the Public Employment Service of Bolivia into formal economic units.[3] The specific objectives were: (i) to strengthen the positioning of the Public Employment Service of Bolivia, and (ii) to improve the effectiveness of delivering services to job seekers who approached the Public Employment Service of Bolivia.

To achieve this, the program offered six-month economic support equivalent to one to two minimum wages for job seekers participating in on-site training. This support was provided for up to three months for the general population, and for up to six months for participants in specific pilot programs targeting people with disabilities, youth, or women in non-traditional occupations. The economic support was aimed at professional adults without relevant work experience and non-professional adults with work experience gained in precarious jobs. These individuals face difficulties in job placement despite having profiles that are in demand for specific positions. SPEBO provides a job induction course, and companies accepting program beneficiaries must submit a training report detailing the on-site training process. From an eligibility standpoint, MTEPS, through SPEBO, offers universal services for job listings and job offers, as well as job counseling and placement. Beneficiaries of on-site training are placed in formal economic units with real vacancies, where a training plan has been agreed upon.

To be eligible, individuals must be at least 18 years old at the time of registration, be Bolivian nationals, meet the requirements of the vacancy, be actively seeking work, and not receive benefits from another employment-related program. No previous work experience is required. Companies wishing to participate in PAE II must have a tax identification number or other legal registration, certifying them as formal economic units. They must propose vacancies that credibly lead to potential permanent hires. Companies can request a limited number of economic supports through PAE II, depending on their size. They are entitled to submit a new application only if at least 25% of previous interventions have resulted in contracts. PAE II includes the following components: (i) A job placement service that facilitates contact between workers and hiring companies; (ii) A monetary stipend that PAE pays during the first three months of on-site training, and for six months for people with disabilities, thus alleviating the company's salary burden; (iii) Specialized on-site training programs for hired staff; (iv) Accident insurance during the training period; (v) A care subsidy received by caregivers of the trainees.caregivers of young children (Ministerio de Trabajo, Empleo y Previsión Social, Estado Plurinacional de Bolivia, 2012).

The procedure for a job seeker joining PAE II is as follows: First, if the job seeker meets the eligi-

[3] While the program accepted job placements in economic units with government registration and recognized that, under the framework of the plural economy proposed by the Constitution of the Plurinational State of Bolivia, these registrations could be of different natures, the majority of economic units that participated in the program had a Tax Identification Number under the general regime. This was because such registration demonstrated their capacity to meet tax obligations and other legal responsibilities, which were considered as approximate indicators of the potential to generate quality employment.

bility criteria, they register and begin the job placement process. This involves matching the job seeker with a vacancy. PAE staff carry out this process by performing a preselection of candidates when a vacancy is published. They prepare a shortlist of around three candidates per vacancy and deliver it directly to employers. Employers contact and interview the preselected candidates and choose one. If the company decides not to continue with the on-site training agreement for any candidate, it does not choose any candidate. Decisions regarding job interviews and job offers are up to the company, while accepting or rejecting the job offer is the responsibility of the job seeker. Once the offer is accepted, PAE oversees the follow-up, manages the administrative tasks related to the program, and makes the monetary stipend payment.

Once the job seeker integrates into a job, they receive on-site training at the company that has accepted them. At the start of the job training, the program develops an induction course for the beneficiaries. This course lasts 12 academic hours, is coordinated with the company, and is conducted by SPEBO. Within the company, the training the job seeker receives is similar to an internship program. The company assigns an experienced person to supervise the beneficiary. The on-site training has a maximum duration of three months and takes place at the company's facilities. Companies hosting the program beneficiaries must prepare a training report detailing the on-site training process. The effective duration of the training for beneficiaries should not exceed 8 hours per day or 48 hours per week.

The job seeker also receives a monetary stipend provided by PAE for three months. This economic support, or job training subsidy, acts as an incentive to cover expenses incurred by the program beneficiaries during the training process. The monthly subsidy amount is determined based on the National Minimum Wage (NMW), the educational level of the beneficiaries, and the sector to which the company belongs. The subsidy amount ranges between one and two minimum wages (Ministerio de Trabajo, Empleo y Previsión Social, Estado Plurinacional de Bolivia, 2021). According to the percentage scale, in the production sector, beneficiaries with a high school education or lower (Category A) receive 120% of the NMW, technicians or mid-level graduates (Category B) receive 160%, and university or higher graduates (Category C) receive 200%. In the service sector, Categories A, B, and C receive, respectively, 100%, 140%, and 180%; while in commerce, they receive 100%, 130%, and 160%. In monetary terms, in production, beneficiaries in Categories A, B, and C receive 2,598, 3,464, and 4,330 Bolivianos, respectively; in services, they receive 2,165, 3,031, and 3,897 Bolivianos; and in commerce, they receive 2,165, 2,815, and 3,464 Bolivianos, respectively, for each category. There is also a complementary temporary benefit of 1,000 Bolivianos for single mothers with children under 5, as well as for widowed fathers and guardians.

At the end of the on-site training period, the monetary support payment ends, and the company must prepare a written evaluation, stating whether the beneficiaries have been integrated or, alternatively, the reasons for not proceeding with the hiring.

3 Data and Descriptive Statistics

In our current evaluation, we have used data obtained from in-person surveys directed at potential beneficiaries of the PAE in Bolivia. These surveys covered both beneficiaries of the program—those who went through the placement process and secured an on-site training agreement—and those who did not benefit from it. We used data from individuals who resort to the employment system, as it is crucial to understand if they use the Public Employment Service or not. The data were collected in two phases: one in 2019 and another during the 2022-2023 period. Through these surveys, we obtained details about the participants' work histories, which facilitated the construction of a monthly panel for evaluation. Initially, every change in the individuals' employment status—such as changes in working conditions, unemployment, or inactivity—was recorded individually. Then, we expanded the data sample based on the duration of each specific employment status, thus constructing a detailed work history for each individual. This methodology compiles information from 2017, despite PAE II being implemented between 2018 and 2022.

Dataset. We have a dataset in unbalanced panel format, covering January 2017 through December 2022. This dataset includes 3.140 individuals and72 periods, with a total of 220.697 observations. In terms of individual characteristics, the average age of the participants was 30,30 years, with a median of 28 years, suggesting a predominantly young sample. Women constitute 54 % of the sample. In addition, the 31 % of the participants are married or cohabiting, 38 % are heads of household and 12 % of respondents speak an indigenous language (see Table A1). With respect to the differences between treated and controls, we see that in the control group there are more women than men (56% vs 48 %), a smaller fraction of heads of household (37% vs. 40%), a higher fraction of people who speak an indigenous language (12% vs. 10%) and a higher proportion of people are married or cohabiting (32% vs. 29%) (see Tables A2 y A3).

Outcome variables: employment and income. We evaluated the impact of PAE II on employment by distinguishing between general employment and employment in formal economic units. General employment was defined as participation in any economic activity during the study period. Employment in formal economic units was defined as employment in units that have a Tax Identification Number (TIN), issue invoices, or have another type of legal registration. The vast majority of companies considered formal had a General Regime Tax Identification Number. Regarding income levels, we used self-reported information on the monthly income of workers. To minimize the influence of outliers in the estimates, we limited our analysis to data between the 5th and 95th percentile of the income distribution, considering only incomes above 400 Bs. per month (58 USD) and below 4,500 Bs. per month (655 USD). The average monthly income of the active workers is of 2.054 Bs. (300 USD), WITH a 57 % of them employed.[4] Of these, only 27 % works in formal economic units (see Table A1).

When analyzing the statistical distribution of the outcome variables, we observed that the vari-

[4]The national minimum wage in Bolivia is 2,362 Bs, according to the adjustment made for the year 2023.

able related to employment presents a more extensive distribution throughout its range of values. In contrast, employment in formal economic units is characterized by a greater concentration of data near zero, as shown in Figure A1. When analyzing PAE II beneficiaries before and after the program (panels (b) and (d) of Figure A1), a change in the employment distributions is detected: an increase in the percentage of employed individuals and a decrease in the percentage of unemployed individuals, both in general and formal employment. This same pattern is evident in the distribution of monthly income, where a shift to the right is observed in Figure A2, suggesting an increase in monthly income.

Treatment variable: being a beneficiary of PAE II. The treatment variable in our study reflects participation in three interventions that are part of PAE II: job intermediation, which connects candidates with firms; the employment subsidy, which provides a monetary stipend for three months; and on-site (or in-plant) training, which offers practical on-the-job training. Individuals who participate in these three interventions—job intermediation, subsidy receipt, and training—are considered the treated group. Specifically, the start of treatment corresponds to the period when the monetary stipend is first received, following job intermediation and one month of on-site training.

The treated group is made up of 865 individuals and 60.649 observations. We note that the treatment is staggered, i.e., the units adopt the treatment at different points in time (see Figure A6). 70% of the units do not receive treatment at any period (see Figure A6, panel (a)), while 30 % receive it at some point (see Figure A6, panel (b)). The highest concentration of events occurs between July 2019 (period 31) and October 2020 (period 46) (see Figure A6, panel (c)). The four periods with the highest number of events are September (period 45), October (period 46), August (period 44) 2020 and September 2019 (period 33). The cumulative frequency up to October 2020 is 60 percent (see Figure A6, panel (d)). This data set presents a hybrid structure, with variability in event dates among treated units and a higher percentage of units that never received treatment (55%).

Control group. The control group consists of 1.029 individual and 160.048 observations. For the formation of the control group in our study, we used data collected through surveys of individuals enrolled in SPEBO. These individuals, who were registered in SPEBO but did not participate in PAE II training programs, make up the control group. We selected these individuals using a detailed, census-like methodology, collecting information from all job seekers who visited the employment offices to register or update their data in the Job Bank. This approach ensured that the control group accurately represented the general population of job seekers, providing a solid basis for our comparisons with PAE II beneficiaries.

Differences between groups and pre-treatment balance. Program beneficiaries have lower employability, both overall and in formal economic units, and although they have higher incomes among the active, these are lower when both active and non-active are considered, indicating a higher proportion of unemployment in this group (see Table A4). In terms of individual characteristics, the treated group has a lower proportion of women, a lower average age and a lower percentage of married or cohabiting individuals. The other characteristics are similar between the two groups. The treated group has an average monthly income of Bs. 2,135 (US$ 311) while the control group has an average monthly income of Bs. 2,021 (US$ 294).[5] The average employment rate is 59% for the treated group, while for the control group it is 57%. Finally, employment in formal economic units is higher for the treated group, at 32%, while for the control group it is 24% (see Tables A2 and A3). These differences are attenuated when comparing the pre-treatment group averages (see Table A4). The employment rate shows a difference of 4 pp, with 53% for the treated group compared to 57% for the control group. In employment in formal economic units, the difference is also 4 pp, with 28% for the treated group compared to 24% for the control group. In terms of income, the treated group recorded an average of 58.7 Bs (8.5 USD) more than the control group.

Time evolution of employment and income. The temporal evolution of the employment and income variables reflect the impact of the pandemic on the economy (see Figure A3). In terms of employment, prior to the pandemic, those treated had a lower employability rate than controls, but this situation is reversed in the recovery phase, being more noticeable in women than in men (see Figures A4 and A5). During the economic recovery phase, PAE II beneficiaries experience an increase in employment compared to non-beneficiaries. With respect to employment in formal economic units, those treated already showed higher levels before the pandemic, a trend that is sustained in the recovery phase for both men and women. In terms of income, throughout the period, the treated group recorded higher monthly incomes, with a more notable difference in women. Post-pandemic income recovery is more significant in beneficiaries compared to non-beneficiaries.

4 Empirical Strategy

The main results of this paper are based on the conventional difference-in-differences model with individual and time fixed effects ($TWFE$) and the estimator of Callaway & Sant'Anna (2021) (DD_{CS}). We supplemented these analyses with a series of event studies, taking into account some of the recommendations from Freyaldenhoven et al. (2021) and Miller (2023). We describe the context of our evaluation based on a conventional $TWFE$ model in order to explain its limitations. Subsequently, we discuss the sensitivity of the conventional $TWFE$ estimator to treatment impact heterogeneity and staggered adoption, and then justify the choice of the Callaway & Sant'Anna (2021) estimator as a complement. Finally, we discuss the validity of the parallel trend assumption and the main robustness checks.

Base specification. The equations (1) and (2) present the models to be estimated within the

[5] The exchange rate applied is 1 U.S. dollar equivalent to 6.86 bolivianos. The exchange rate has not fluctuated significantly during the evaluation period.

framework of a conventional $TWFE$ model, in their static and dynamic versions, respectively.

$$Y_{it} \;=\; \nu_i \;+\; \rho_t \;+\; \beta \cdot D_{it} \;+\; X_{it} \cdot \gamma \;+\; \varepsilon_{it} \tag{1}$$

$$Y_{i,t} \;=\; \nu_i \;+\; \rho_t \;+\; X_{it} \cdot \gamma \;+\; \sum_{e=-K}^{-2} \delta_e^{\text{anticip}} \cdot D_{i,t}^e \;+\; \sum_{e=0}^{L} \beta_e \cdot D_{i,t}^e \;+\; v_{i,t} \tag{2}$$

Y_{it} denotes the individual's outcome variable i in the period t. The outcome variables are employment, employment in formal economic units, monthly income (in levels) and monthly income (in logarithms). The variable D_{it} is dichotomous and takes the value of one from the first month in which the individual benefits from the program, thus marking the beginning of treatment in the period in which they first receive the monetary stipend. The term v_i represents an individual fixed effect, while ρ_t is a time fixed effect. The incorporation of individual fixed effects allows controlling for time-invariant but variable characteristics across individuals, while the time fixed effect ensures that the results are not solely attributed to a time trend. X_{it} represents the set of control variables: age, relationship to the head of household, gender and the use of an indigenous language. The term ε_{it} corresponds to the standard error term. In the equation (1), the coefficient $\hat{\beta}$, is obtained by Ordinary Least Squares (OLS). As usual, standard errors are clustered at the individual level (treatment level) allowing for serial correlation (Bertrand et al., 2004). In the equation (2), $D_{i,t}^e = \mathbb{1}\{t - G_i = e\}$ is an indicator variable that the unit i is at e periods of time away from the initial treatment t. K and L are positive constants. The variable of interest in this case corresponds to $\{\beta_e : e \geq 0\}$, these parameters being interpretable as the effect of program participation on the outcome variable Y_{it} at different durations of treatment exposure.

The identification of the causal impact of the program rests on three assumptions. The first is the assumption of parallel trends. This assumes that, in the absence of the program, the trajectories of income, employment and employment in formal economic units of the beneficiaries would have followed a course identical to that of those who did not receive the benefit. The second assumption, the non-anticipation assumption, holds that if a unit does not receive treatment in period t, its outcome is not influenced by the possibility of receiving treatment in future periods. This implies that treatment has no causal effect prior to its implementation and that it is necessary to complete program participation in order to effectively increase the likelihood of accessing formal employment or employment, or increase earnings. Finally, it is assumed that the average impact of the treatment is constant across treated units and over time. Under these assumptions, it is possible to state that $\hat{\beta}$ and $\hat{\beta}_e$ acquire a causal interpretation. That is, the observed results are a direct consequence of participation in the program and are not due to other factors contemporaneous with its implementation.[6]

[6] An alternative view of the identification assumptions in a $TWFE$ model has to do with the additive structure of the potential outcome variables (*potential outcomes*) in the absence of treatment. From the base specification it follows

Recent literature on $TWFE$ models indicates that it is necessary to be cautious in interpreting the parameter estimates in such models as a causal effect, especially if we suspect that the impact of treatment is not constant across groups and over time (De Chaisemartin & d'Haultfoeuille, 2020; Callaway & Sant'Anna, 2021; Sun & Abraham, 2021; Roth et al., 2023). In fact, it is to be expected that most economic phenomena exhibit heterogeneity in the impacts of a policy (Freyaldenhoven et al., 2021). Moreover, even if we consider that the above problem does not exist, there may be additional problems in stepwise treatment contexts that induce bias in the estimates or make it difficult to interpret the estimator (Goodman-Bacon, 2021). In addition, the $TWFE$ estimator tends to estimate an average treatment impact that overestimates short-term effects and underestimates long-term effects (Borusyak & Jaravel, 2018). In our case, where the program includes a subsidy during the first three months that then ceases, it is relevant to address this bias, since the $TWFE$ could be estimating a higher average treatment impact than the actual one. Based on these facts, we choose as a complementary estimator to the one proposed by Callaway & Sant'Anna (2021) as it is robust to these problems, however, the results of this evaluation must take into account the set of reported estimators and the proposed robustness exercises.

Sensitivity of estimator. If the treatment impact is heterogeneous over time or between groups, the conventional $TWFE$ estimator may be biased (Roth et al., 2023; Goodman-Bacon, 2021; De Chaisemartin & d'Haultfoeuille, 2020). To diagnose the presence of heterogeneity of treatment impact, we calculated the "negative weights" based on the following De Chaisemartin & d'Haultfoeuille (2020). The calculation of these weights is based on the fact that the estimator β_{twfe} is a weighted average of the average treatment impact of each treated cell where the weights can be negative. Formally, $E[\widehat{\beta}] = E\left[\sum_{g,t} W_{gt}\Delta_{g,t}\right]$ where W_{gt} are the weights that add up to one and $\Delta_{g,t}$ is equal to the average impact of the treatment on the g group in the period t. The calculation of negative weightings is justified since $E[\hat{\beta}]$ may be negative even if all average treatment impacts are positive. In addition, negative weights are more likely to be assigned to periods with a higher proportion of treated groups, or to groups treated in many periods. Negative weights are a problem when the treatment impact differs between periods with many vs. few treated groups (case of staggered treatment adoption), or between groups treated for many periods vs. few periods.

To diagnose the impact of staggered treatment adoption on the $TWFE$ estimator, we decompose the $TWFE$ estimator according to Goodman-Bacon (2021). This exercise allows us to quantify how much of the variation that identifies the treatment impact comes from comparisons between treated units and pure controls, and how much is due to temporal variation in treatment adoption. In the presence of multiple groups and staggered treatment adoption, the estimator $\hat{\beta} twfe$ is a

that: $E\left[Y_i \mid D_i = 0, t\right] = \nu_i + \rho_t$. Two implicit assumptions arise from this: (i) If there are pre-existing differences in levels between the treatment and control groups, these differences are time-varying and do not affect the slope of the time trend; (ii) If there are pre-existing differences in levels between the treatment and control groups, these differences are time-varying and do not affect the slope of the time trend (ν_i). That is, they are additive and not multiplicative; (ii) In the absence of the treatment, the outcome variable for both the control and treatment groups should follow the same trend (rho_t). Thus, the control group can be used to estimate the time counterfactual.

weighted average of all possible comparisons between groups over time. The weights depend on the size and variance of the treated groups. Groups with more units and/or those treated in the middle of the period have a larger weight. Treated units can act as both controls and treatments depending on the comparison type, which can induce $\hat{\beta}twfe$ to be a biased estimator. The bias comes from making comparisons between previously treated units and those treated later, generating "prohibited comparisons". For example, a worker who completed the program in 2018 could be incorrectly used as a control for another worker who began participation in 2020. This bias manifests itself if the average treatment impact for the "already treated" group differs over time. Otherwise, there is only potential bias arising from the failure to meet the assumption of parallel trends, i.e., from selection bias. The results of these exercises are presented in Section6.

Callaway & Sant'Anna (2021) estimator.[7] This estimator is suitable for our context since it avoids the presence of negative weights, allows the researcher to specify how the effects are weighted across cohorts (e.g., proportional to the size of the cohort) instead of being determined by OLS, where the weighting is proportional to the variance of the treatment variable, and is explicit in indicating which units are being used as the control group, which is especially useful when there is staggered treatment adoption. The design of this estimator contemplates a series of aggregations of the average impact of the treatment, among which we can mention: the time of exposure to the treatment, the differences between treated groups and the cumulative impact over time. These aggregations provide information on the temporal evolution of the impact on different treated groups, allow us to examine the heterogeneity between these groups and to understand the impact of the program on each group during a specific period. Thus, the resulting set of estimators provides a more complete and detailed perspective than the conventional estimator.

The average treatment impact for units that are treated for the first time in period g during period t is represented by the equation (3). $ATT(g,t)$ represents the average treatment impact in period t for those who received the monetary stipend for the first time in g.

$$ATT(g,t) = \mathbb{E}\left[Y_t(g) - Y_t(\infty) \mid G_i = g\right] \qquad \forall g \geq t \tag{3}$$

Under the assumption of parallel trends and no anticipation, it is possible to identify the causal effect of the program by comparing the expected change in the outcome variable for group g between periods $g-1$ and t, with a control group composed of units that have not yet received treatment in period t (Callaway & Sant'Anna, 2021; Roth et al., 2023). This comparison remains valid when averaging over a set of cohorts $g \in \mathcal{G}_{\text{comp}}$ such that $g > t$.[8] Equation (4) shows this comparison.

[7] This subsection closely follows the discussion in Callaway & Sant'Anna (2021) y Roth et al. (2023)

[8] It is possible to calculate the estimator using two control group options: (i) only considering untreated units; (ii) considering all units not yet treated. The estimates presented here only consider units that have never been treated.

$$ATT(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}\right]. \tag{4}$$

By replacing the population means with the sample averages in equation (4), we can obtain the desired estimator for the average treatment impact in each group and period, as indicated by equation (5).

$$\widehat{ATT}(g,t) = \frac{1}{N_g}\sum_{i:G_i=g}\left[Y_{i,t} - Y_{i,g-1}\right] - \frac{1}{N_{\mathcal{G}_{comp}}}\sum_{i:G_i\in\mathcal{G}_{comp}}\left[Y_{i,t} - Y_{i,g-1}\right] \tag{5}$$

As mentioned above, the estimator also allows us to obtain a number of additional measures to the aggregate treatment effect, as indicated by equation (6).

$$\theta = \sum_{g\in\mathcal{G}}\sum_{t=2}^{T} w(g,t)\cdot \text{ATT}(g,t) \tag{6}$$

where $w(g,t)$ correspond to specific weights that allow measuring different types of treatment in a policy.[9] In our context, they allow us to answer, for example, the following questions: (a) How does the effect of participating in treatment vary with the duration of treatment exposure? (b) Do groups that are treated earlier have, on average, larger/ smaller treatment effects compared to groups that are treated later? (c) What is the average cumulative treatment effect of the policy across all groups up to a particular point in time? Thus, this estimator is adaptable to an event study that delivers the weighted average treatment impact l periods after adoption across different adoption cohorts, as indicated by eq. (7).

$$ATT_l^w = \sum_g w_g ATT(g, g+l) \tag{7}$$

Parallel trends. To assess the robustness of the parallel trend assumption, we carried out two exercises. First, given that $\delta_e^{\text{anticip}}$ and β_e capture the dynamic impact of the treatment, we test whether there are differences in the trajectories of the treatment and control groups prior to treatment. It is important to note that this is only one "partial test" since this assumption requires parallel trajectories in the absence of the treatment, which is not possible to observe. Second, we calculate the synthetic control estimator for differences in differences in differences ($SCDD$) proposed by Arkhangelsky et al. (2021). We present aggregate estimators and associated event studies for the employment variables in the case of the $SCDD$ estimator. These estimators allow us to relax the assumption of parallel trends by generating a synthetic counterfactual that aligns the pre-treatment trends optimally based on individuals' employment history.

[9] For more details on the different types of weightings and aggregations, see Table 1 in Callaway & Sant'Anna (2021).

Synthetic Difference-in-Difference Estimator. According to Arkhangelsky et al. (2021), the goal is to obtain a consistent estimator of the causal effect of a policy (or treatment W_{it}) even when we do not believe that the assumption of parallel trends is met. To obtain an estimator of the average impact we proceed by estimating the following equation:

$$\left(\tau_a^{\text{sdid}}, \hat{\mu}_a, \hat{\alpha}_a, \hat{\beta}_a\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_{a,i}^{\text{sdid}} \, \hat{\lambda}_{a,t}^{\text{sdid}} \right\} \tag{8}$$

The average treatment impact estimator for the treated is generated from a regression of individual and time fixed effects, with weights ω_{sdid}^{i} y λ_{sdid}^{t} optimized. According to Clarke et al. (2023), this procedure allows for the presence of time-shared aggregate factors, due to the estimation of the time fixed effects β_t, and time-invariant unit-specific factors, due to the estimation of the unit fixed effects α_i. The presence of unit fixed effects implies that the synthetic difference-in-differences estimator will seek to match treatment and control units on pretreatment trends, and not necessarily on pretreatment trends and levels. In this way, the estimator allows for the existence of different levels between treatment and control units prior to treatment, thus differing from traditional synthetic control estimators. Unlike the traditional synthetic control estimator, the $CSDID$ not only weights the units in the control group, but also the pre-intervention time periods, in order to approximate the counterfactual.

Arkhangelsky et al. (2021) indicates that the estimation process can be adapted to the case of staggered treatment, where units are treated at different points in time. In this case, the average treatment impact is calculated by applying the synthetic difference-in-differences estimator to each of the subgroups treated at different points in time. Then, a weighted average is calculated for each of the subgroups based on the number of treated units and periods that each subgroup has that adopt a treatment at some point in time. As indicated by Clarke et al. (2023), the process of estimating the average treatment impact, in the case of phased adoption, is based on the following algorithm:

1. We have data for the outcome variable (Y), the matrix indicating which units are treated per period (W) and the row vector containing the different adoption periods between units (A).
2. Then, for each $a \in A$:
    - A subset of Y and W is selected for units that are pure controls and for those who adopted the treatment at $t = a$. regularizing parameter is computed: ζ.
    - The weights are calculated for each unit: $\hat{\omega}_a^{\text{sdid}}$.
    - Weightings are calculated for each unit: $\hat{\lambda}_a^{\text{sdid}}$
    - The synthetic difference-in-difference estimator is calculated according to the following equation:

$$\left(\tau_a^{\text{sdid}}, \hat{\mu}_a, \hat{\alpha}_a, \hat{\beta}_a\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_{a,i}^{\text{sdid}} \, \hat{\lambda}_{a,t}^{\text{sdid}} \right\} \tag{9}$$

3. Once the above process has been completed, the average treatment impact is calculated according to the following equation:

$$\widehat{ATT} = \sum_{\forall a \in \mathbf{A}} \frac{T_{\text{post}}^a}{T_{\text{post}}} \times \hat{\tau}_a^{\text{sdid}} \tag{10}$$

   Where T_{Post} is the total number of post-treatment periods observed in the treated units.

Event study (SCDD). The synthetic control estimator in difference-in-differences can be viewed as an event study that allows us to observe how the treatment effect changes over time, as well as the differences between treated and control units prior to treatment implementation.

Although the standard visualization allows us to see trends for both groups, it does not provide a clear picture of how differences in outcome variables evolve compared to pretreatment differences and whether these differences are statistically significant. In order to visualize the event studies associated with this estimator, we calculated for each period the following:

$$(\bar{Y}_t^T - \bar{Y}_t^C) - (\bar{Y}_b^T - \bar{Y}_b^C) \tag{11}$$

Where $\bar{Y}_t^T$ represents the average value of the observed outcome variable for the treated units and $\bar{Y}_t^C$ for the synthetic control units. The base values are determined by the following equations:

$$\bar{Y}_b^T = \sum_{t=1}^{T_{pre}} \hat{\lambda}_t^{sdid} \bar{Y}_t^T \qquad \bar{Y}_b^C = \sum_{t=1}^{T_{pre}} \hat{\lambda}_t^{sdid} \bar{Y}_t^C \tag{12}$$

$\hat{\lambda}_t^{sdid}$ is the optimal time-weight derived by obtaining the synthetic difference-in-differences control estimator. Thus, the observed baseline differences between treated and controls are a weighted average of the outcome variable before treatment. Thus, based on one year of treatment implementation, we can calculate in each period whether the difference between the treated and synthetic units has changed compared to the baseline differences, along with the corresponding confidence interval[10]. Confidence intervals are calculated with *bootstrap* considering 1,000 repetitions.

[10] If there were only one treatment period, we would have only one graph associated with the event study. By having a staggered treatment, we can generate an event study for each treatment adoption period.

5 Results

Main results. Tables 1 and 2 show a summary of the program's results on employment, employment in formal economic units and monthly income both in levels and in logarithms (active only). We present the estimators of $TWFE$ with and without controls and the estimator of Callaway & Sant'Anna (2021) assuming unconditional parallel trends. Table 3 shows the impact of the program at different degrees of treatment exposure based on the unconditional DD_{cs} estimator.

Using the conventional unconditional estimator (TWFE), we find that the program increases employment by 14.6 percentage points (pp.), with a specific increase of 14.0 pp. in men and 14.8 pp. in women. With respect to employment in formal economic units, we observed an increase of 14.9 pp., 14.9 pp. for men and 14.3 pp. for women. Regarding monthly income, there was an increase of 128 bolivianos (19 USD), broken down into 111 Bs (16 USD) for men and 155 Bs (22 USD) for women. Compared to the control group, the percentage increase in income is 9.5 percent, with 8.1 percent for men and 11.7 percent for women (see Tables A5, A7, y A9). Comparing these results with the pre-treatment averages, we find that the normalized impacts are: 41% in employment (0.146/0.35), 87% in employment in formal economic units (14.9/17.0) and 6.7% in income in levels (128/1,907). We observed that the differences between the treated and control groups in the case of income are minimal and centered on zero, while for the employment variables a previous trend is observed (see Figures 2, A7 and A8). The results of the $TWFE$ estimation with controls report results of a similar order of magnitude to the unconditional model (see Tables A6, A8, y A10).

According to the estimator of Callaway & Sant'Anna (2021), employment increases by 21 percentage points (pp.), with a specific increase of 19.7 pp. for men and 21.7 pp. for women. With respect to the impact on employment in formal economic units, we observe that the increase is 17.9 pp., broken down into 17.8 pp. for men and 17.5 pp. for women. In terms of income, there is an increase of 282 Bs. (41 USD), being 244 Bs. (35 USD) for men and 350 Bs. (50 USD) for women. In percentage terms with respect to the control group, the increase in monthly income is 15.7%, with 21.0% for men and 12.8% for women. Comparing these results with the pre-treatment averages, we find that the normalized impacts are: 60.0% in employment (0.21/0.35), 105.0% in employment in formal economic units (17.9/17.0) and 14.7% in increase in income (282/1907) (see Tables A15, A16, A17, and A18). We observed no differences between the treated and control groups prior to treatment, as shown in each event study in Figure 1.

The results indicate that the program has a significant and economically relevant impact on both employment and formal employment, regardless of the estimator used. Regarding income, the results are positive and statistically significant, however, although we identify a positive and significant impact in most estimates, this is attenuated when adjusting for prior trends and observable variables. The $SCDD$ estimators in the robustness section, confirm a significant impact on employment and employment in formal economic units (see Table A27), but the results are inconclusive in terms of income, partly due to the requirement for a balanced panel (see Table A28). The main results of this paper are based on the conventional difference-in-difference estimator, which yields more conservative figures than the estimator proposed by Callaway & Sant'Anna (2021). This suggests that the impact of the program we present is at least a conservative estimate of the true impact.

Heterogeneity of treatment impact. Table 3 details the impact of the program at various time horizons after the start of treatment. It is found that the positive effects on both employment and income persist up to two years after the start of the program, although their magnitude decreases over time. This trend is also reflected in Figure 1, which illustrates the impact of the program as a function of the duration of exposure to the treatment. In particular, we observe a marked short-term effect on employment and employment in formal economic units, an impact that is significantly reduced at the end of the on-site training phase (at $t = 4$), and then reaches a stabilization phase. On the other hand, the impact on income shows greater volatility, as evidenced both in the estimators used and in the confidence intervals. However, this impact on income remains positive and statistically significant up to two years after the start of treatment.

The impact of treatment not only varies with the degree of exposure, but also between treated groups. The Figures A12 and A13 show event studies associated with the $SCDD$ estimator for the employment variables for the four most relevant treated groups. These figures allow us to observe the heterogeneity of treatment impact among treated groups at different points in time. For both the employment and formal employment cases the results suggest that the group treated in September 2019 has lower employment performance than the groups treated in other periods, where the positive impact of the program remains stable. In this event studies we also observe a strong impact in the short term, decreasing, but positive in the medium term.

Size of treatment impact. Our results show a greater impact than that observed in the PAE I evaluation. Novella & Valencia (2022) report that PAE I increased the probability of being employed by 8 pp. and the probability of being employed in a formal economic unit by 4 pp. With respect to income, they report an increase of 9% relative to the control group, although not statistically significant. Our estimates ($TWFE$ with controls) show a higher impact on both employment (14.3 pp.) and formal employment (14.6 pp.) and a similar impact on income (8.7%). The estimators from Callaway & Sant'Anna (2021) shows an impact in employment (21 pp.), formal employment (17.9 pp.), and income (17.9 pp.). Additionally, the increase in income is even greater (15.7%). In order to make an accurate comparison with the results obtained in the PAE I impact evaluations, we have carried out estimations using both the conditional and unconditional forms of the estimator (see A29, A30, A31 y A32), as the estimator of CS (Abadie et al., 2010) (see A33 and A34), but applying these estimators to the data collected in the PAE I evaluation.

The conventional conditional estimator $TWFE$ shows an increase in employment of 11.2 pp., broken down into 10.3 pp. for men and 12.3 pp. for women. Employment in formal economic units increases by 5.9 pp., being 5.3 pp. for men and 6.6 pp. for women. Income, measured in levels, increases by 219 Bs. (equivalent to 19 USD), with an increase of 122 Bs. (equivalent to 17 USD) for men and 308 Bs. (equivalent to 44 USD) for women. However, this increase is statistically significant at 10 percent only when the entire sample is considered. In percentage terms with respect to the control group, monthly income grows by 2.4 percent, with 0.5 percent for men and 4.1 percent for women, this result being significant at 5 percent considering the entire sample. The CS estimators reflect a positive impact on employment of 9 pp. (9.3 pp. for men and 8.4 pp. for women). Employment in formal economic units rises by 5.9 pp. (6.2 pp. for men and 5.3 pp. for women). Income, measured in levels, increases by 243 Bs. (35 USD), with 165 Bs. (24 USD) for men and 300 Bs. (43 USD) for women. In percentage terms with respect to the control group, we observed an increase in monthly income of 7.2% (4.2% for men and 8.9% for women). Our findings, both in employment and income, exceed those reported by PAE I, regardless of the type of estimator used. This is particularly evident in the employment variables and in the case of the $TWFE$ estimator for income.

Table 1. Estimators of: $TWFE$ and $CSDD$ for Employment Variables

	All	Women	Men	All	Women	Men
	Employment (pp.)			**Formal Employment (pp.)**		
Method A – $TWFE$: Unconditional						
PAE Beneficiary	0.146***	0.148***	0.140***	0.149***	0.149***	0.143***
	(0.016)	(0.024)	(0.021)	(0.017)	(0.024)	(0.024)
Observations	220,697	118,537	102,160	220,697	118,537	102,160
Method B – $TWFE$: Conditional						
PAE Beneficiary	0.143***	0.146***	0.137***	0.146***	0.144***	0.142***
	(0.016)	(0.024)	(0.021)	(0.017)	(0.024)	(0.024)
Observations	220,697	118,537	102,160	220,697	118,537	102,160
Method C – DD_{CS}**: Callaway & Sant'Anna (2021)**						
PAE Beneficiary	0.210***	0.217***	0.197***	0.179***	0.175***	0.178***
	(0.019)	(0.027)	(0.029)	(0.018)	(0.026)	(0.026)
Observations	220,696	102,128	118,491	220,697	102,128	118,491

Notes: Each cell presents results from an individual, reduced-form estimation varying the estimation procedures (rows). The $TWFE$ models are in the first two rows, with the second model considering control variables. DID_{CS} refers to grouped estimations (all groups in all periods) based on Callaway & Sant'Anna (2021). Standard errors are clustered at the individual level. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2. Estimators of: $TWFE$ and DD_{CS} for Income Variables

	ALL	WOMEN	MEN	ALL	WOMEN	MEN
	Income (Levels)			**Income (Logs)**		
Method A – $TWFE$: Unconditional						
PAE Beneficiary	128.563***	155.256**	111.472*	0.095**	0.117***	0.081**
	(44.933)	(62.339)	(63.511)	(0.026)	(0.039)	(0.035)
Observations	110,791	55,555	55,236	110,791	55,555	55,236
Method B – $TWFE$: Conditional						
PAE Beneficiary	117.486***	144.028**	98.716	0.087***	0.106***	0.074**
	(45.236)	(62.943)	(63.619)	(0.026)	(0.039)	(0.035)
Observations	110,791	55,555	55,236	110,791	55,555	55,236
Method C – DD_{CS}**: Callaway & Sant'Anna (2021)**						
PAE Beneficiary	282.074***	350.450***	244.160***	0.157***	0.21***	0.128***
	(70.031)	(122.954)	(84.781)	(0.041)	(0.078)	(0.047)
Observations	100,001	49,416	50,503	100,001	49,416	50,503

NOTES: Each cell presents results from an individual, reduced-form estimation varying the estimation procedures (rows). The $TWFE$ models are in the first two rows, with the second model considering control variables. DID_{CS} refers to grouped estimations (all groups in all periods) based on Callaway & Sant'Anna (2021). Standard errors are clustered at the individual level. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 3. Combined Impact at Different Moments of Exposure to Treatment

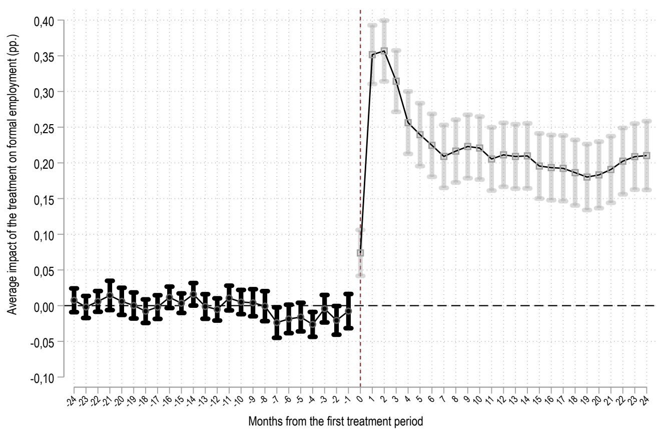
Moment of Exposure	Employment	Formal Employment	Income (Logs)	Income (Levels)
6 MONTHS	0.256***	0.221***	0.222***	369***
	(0.022)	(0.020)	(0.040)	(69)
12 MONTHS	0.221***	0.183***	0.187***	326***
	(0.023)	(0.021)	(0.047)	(82)
18 MONTHS	0.193***	0.161***	0.130*	245**
	(0.023)	(0.022)	(0.053)	(87)
24 MONTHS	0.202***	0.167***	0.168**	311**
	(0.024)	(0.022)	(0.058)	(95)

NOTES: Each cell presents the treatment impact for employment and income variables according to the estimators based on Callaway & Sant'Anna (2021). The estimators reflect the program impact at different moments of treatment exposure. These estimators correspond to those observed in the event study of Figure 1. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

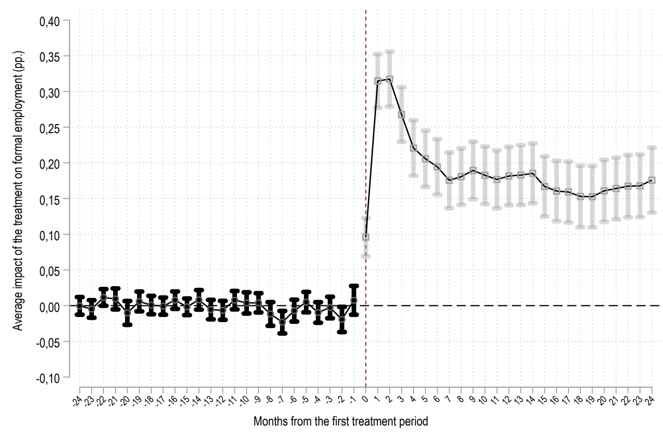
EVENT STUDIES. Figure 1 illustrates the impact of the program at different degrees of treatment exposure based on the estimator of Callaway & Sant'Anna (2021). Figure 2 illustrates the impact of the program at different degrees of exposure based on the conventional difference-in-difference model estimator.

Figure 1. Dynamic treatment impact - estimator DD_{cs}

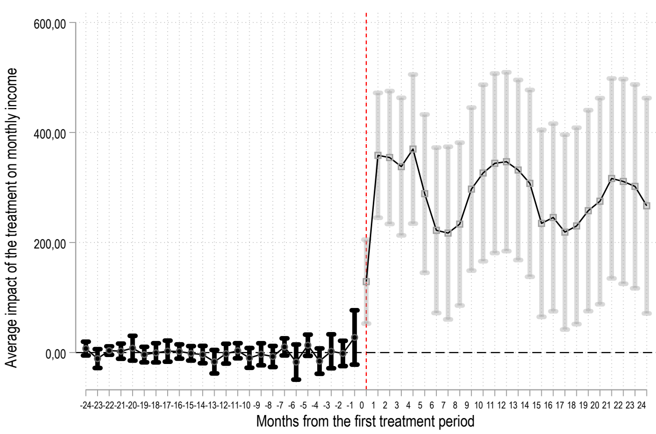
(a) Employment



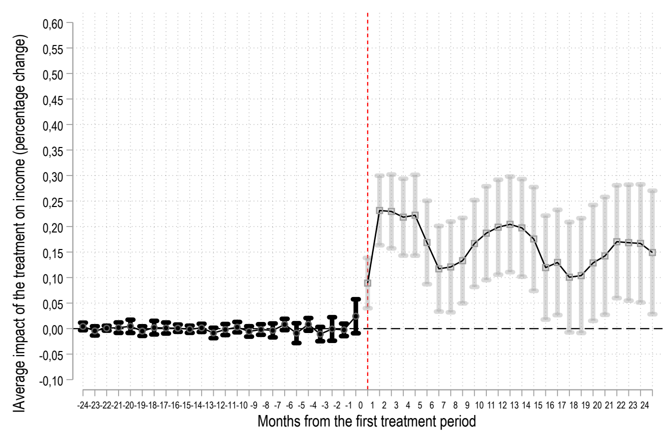
(b) Employment in formal economic units



(c) Income (in levels)

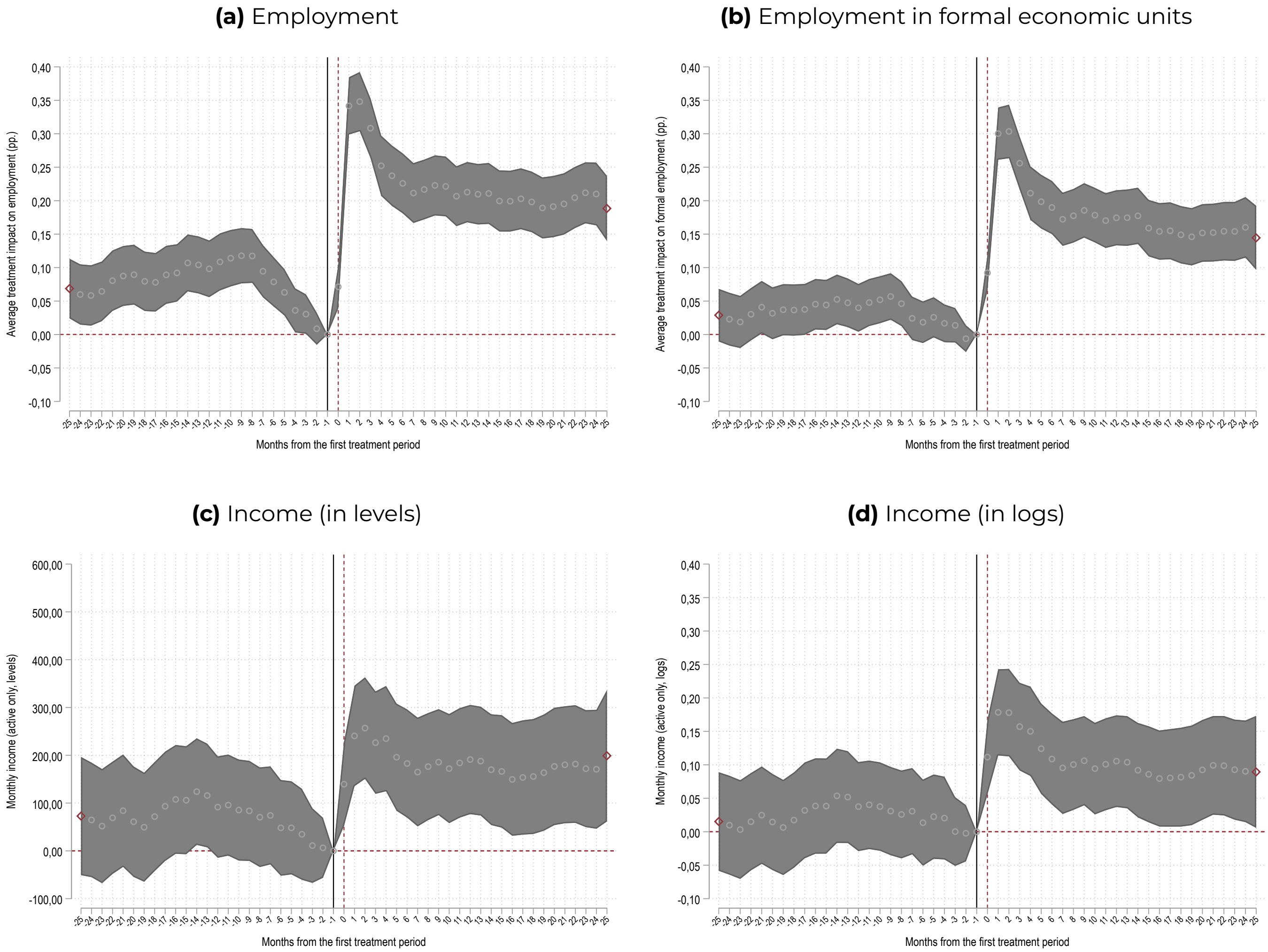


(d) Income (in logarithms)



NOTES: Panels (a) and (b) present the impact of the treatment according to the time elapsed since adoption for the employment variables while panels (c) and (d) show the impact of the program at different degrees of exposure to the treatment for the income variables. The estimation assumes unconditional parallel trends and presents standard errors clustered at the individual level.

Figure 2. Dynamic impact: event study based on $TWFE$ for employment, employment in formal economic units and monthly income (in levels and logs).)



NOTES: Event study based on estimation of the equation (2) controlling for individual characteristics. Standard errors are clustered at the individual level. The impact is normalized to the time immediately prior to treatment adoption prior to the adoption of the treatment. Periods -25 and 25 show the estimates for all periods beyond the 24-month window, representing long-term impacts.

6 Robustness and sensitivity checks

Negative weights. We calculate the negative weights according to the proposal by De Chaisemartin & d'Haultfoeuille (2020) for each $\hat{\beta}_{TWFE}$. The results suggest that the estimator is robust to the presence of treatment impact heterogeneity in the employment case (0.000) and employment in formal economic units (0.000). In the case of income, there is a percentage of negative weights for income in the following levels (-0.008) and income in logarithms (-0.008). However, both are quite small. It is possible to conclude that $\hat{\beta}_{TWFE}$ is robust to the presence of heterogeneity.

Goodman-Bacon (2021) Decomposition. For all outcome variables, the conventional $TWFE$ model estimators are mostly based on comparisons between treated units and pure controls. A small fraction, always less than 3 percent of the variation, is based between later treated units (used as treatment) and earlier treated units (used as controls) (see Figure A9 and Tables A11, A12, A13 and A14). These results suggest that staggered treatment adoption should not be a major problem when considering estimates from the $TWFE$ estimator.

Parallel trends. For each event study of the $TWFE$ estimator (see Fig. 2) we test the joint significance of all the terms simultaneously to be zero based on the hypothesis ($K < 0$): $H_0 : \beta_K = \beta_{-24} = \quad = \beta_{-1} = 0$ vs. $H_1 : H_0$ does not hold. The null hypothesis is rejected for employment (*p-value:* 0,000). It is not possible to reject the null hypothesis for formal employment. (*p-value:* 0,078), for monthly income in levels (*p-value:* 0,505) and for income in logarithms (*p-value:* 0,491). If the null hypothesis is rejected, it is evidence against the assumption of parallel trends; otherwise, it is evidence in favor. Visual inspection of the event studies are consistent with these results (see Figures 2, A7 and A8)[11]. The event study associated with the estimator of Callaway & Sant'Anna (2021) suggests that there is no relevant time trend prior to the adoption of the treatment, however, when performing a formal test we see that in all cases the null hypothesis that all pre-treatment estimators are equal to zero is rejected (see Tables A23, A24, A25 and A26).

Synthetic Control Estimators of differences in differences. The existence of systematic differences between the treatment and control groups prior to the start of treatment may compromise the assumption of parallel trends. The $SCDD$ estimator offers an alternative to relax this assumption since it standardizes the choice of control group units through a procedure based on observed data. The selection of control units is based on the idea that the pre-intervention

[11] It is important to note that the assumption of parallel trends may be questionable in our context due to various factors. Although this assumption allows for the inclusion of variables that affect treatment participation, these variables must have a constant and additive effect on the average outcome over time in order to be controlled for fixed effects. However, if these factors change in a non-additive manner over time, bias will be introduced into the estimates. In our case, there may be different reasons to question this assumption. For example, there may be macroeconomic factors that vary non-additively over time, e.g., pandemic impacts. In addition, it is important to consider the sensitivity of the functional form used in the analysis. For example, while parallel trends may hold in levels, they may not hold if the outcome variable is measured in logarithms (or vice versa). For more details on possible violations of the parallel trends assumption, see: Roth et al. (2023).

outcome variables should be as similar as possible between the control and treated groups. In our context, this method allows the selection of control units with similar work histories to those who participated in the program. One problem with this estimator is that it requires a balanced panel. This is especially problematic for the earnings case, where the number of observations is significantly reduced.

Based on this new balanced panel, we recalculated the conventional difference-in-differences estimator so that they are comparable with the $SCDD$ estimator. The results obtained through these estimators highlight a statistically significant and economically important impact of PAE II on employment. However, these same results do not robustly confirm the impact of the program on earnings, however, it is important to approach this finding with caution, as the requirements of a balanced panel could influence the results. Despite the above, the results in the specification in logarithms are positive, although not statistically significant. Additionally, the results of the $SCDD$ estimator corroborate the temporal pattern already observed: a strong short-term program effect that diminishes over time, but still remains positive up to 24 months after treatment initiation (see Figures. A12 y A13).

Table 4. Estimates of $TWFE$ and $CSDD$ - Employment (by Gender)

	Employment			**Formal Employment**		
	All	Women	Men	All	Women	Men
Method A – $TWFE$: Unconditional						
PAE Beneficiary	0.126***	0.156***	0.089***	0.144***	0.171***	0.117***
	(0.026)	(0.040)	(0.034)	(0.027)	(0.040)	(0.038)
Method B – $CSDD$: Arkhangelsky et al. (2021)						
PAE Beneficiary	0.139***	0.157***	0.121***	0.168***	0.174***	0.158***
	(0.032)	(0.039)	(0.041)	(0.023)	(0.039)	(0.033)
Observations	91,872	48,488	43,384	91,872	48,488	43,384
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Treated Group Mean	0.36	0.35	0.37	0.18	0.16	0.19

NOTES: Each cell presents results from an individual, reduced-form estimation varying the estimation procedures (rows). The $TWFE$ model is in the first row, with the second model considering the estimator proposed by Arkhangelsky et al. (2021). The sample significantly reduces the number of observations, given that the $CSDD$ estimator requires a balanced panel. Standard errors are clustered at the individual level. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 5. Estimates of $TWFE$ and $CSDD$ - Income (by Gender)

	Income (Levels)			**Income (Logs)**		
	All	Women	Men	All	Women	Men
PAE Beneficiary	-110.910	322.456	-517.317	0.143	0.257	0.039
	(623.170)	(715.055)	(1023.922)	(0.348)	(0.513)	(0.493)
Method B – $CSDD$: Arkhangelsky et al. (2021)						
PAE Beneficiary	191.019	-6.166	31.616	0.341	-0.050	0.280
	(656.417)	(802.922)	(1140.569)	(0.369)	(0.663)	(0.491)
Observations	1,890	840	1,050	1,890	840	1,050
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Treated Group Mean	1,903	1,386	2,420	7.34	7.11	7.57

NOTES: Each cell presents results from an individual, reduced-form estimation varying the estimation procedures (rows). The $TWFE$ model is in the first row, with the second model considering the estimator proposed by Arkhangelsky et al. (2021). The sample significantly reduces the number of observations, given that the $CSDD$ estimator requires a balanced panel. Standard errors are clustered at the individual level. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

7 Conclusion

This study emphasizes the relevance of Active Labor Market Policies, particularly in settings where labor informality is high and labor market access barriers are significant. The effectiveness of PAE II, with its focus on direct on-the-job training, subsidies during the on-the-job training process, and labor intermediation services, demonstrates that well-structured programs can have a considerable impact on workers' employability and earnings.

The results of the study suggest that PAE II is especially effective in reducing barriers to accessing formal employment, with a markedly positive impact among women, thus highlighting its ability to address gender disparities in access to employment. It is important to recognize that, although PAE II has generated positive effects on employment, the results in terms of income, although favorable, are not as strong. This finding does not diminish the value of the program; on the contrary, it underscores the need for further research on how improvements in design or implementation can be reflected in sustained increases in income for beneficiaries. In addition, this program was implemented during a critical period of the pandemic and, despite challenges, has demonstrated positive outcomes in terms of employment and income for beneficiaries.

Our findings support existing research on the effectiveness of job training and support programs and underscore their crucial role in improving the labor market inclusion of vulnerable groups. The robustness of these results, confirmed through various empirical methodologies, contributes significantly to the understanding of the impact of PAE II. This study not only contributes to the understanding of the impact of this program but also offers valuable lessons for the design, implementation, and evaluation of future employment policies in Bolivia and the region.

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Appendix

Active labor market policies in Bolivia: impact of the Program to Support Employment II

Nicolás Campos, Manuel Urquidi

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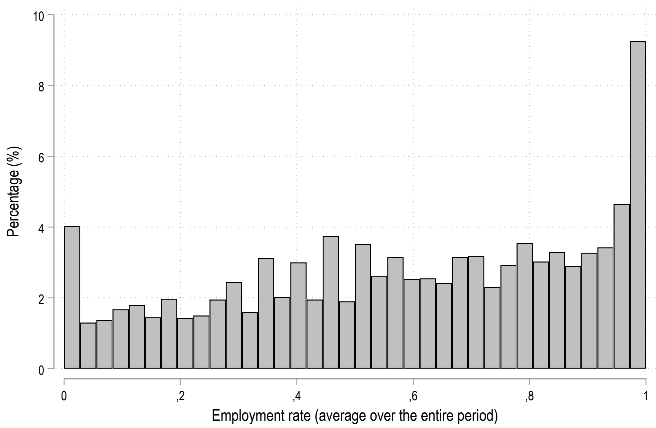
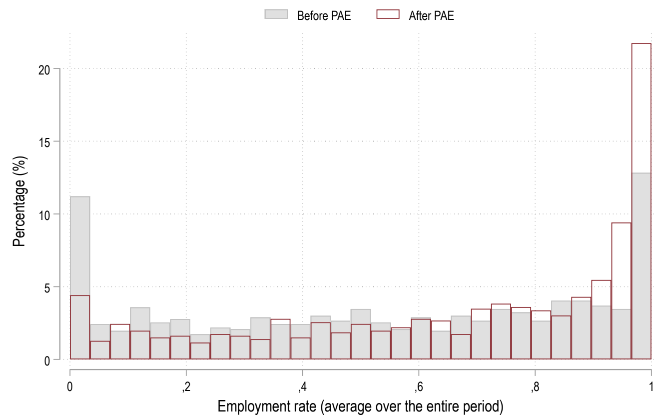
A Appendix: Figures

Figure A1. Statistical distributions of employment variables

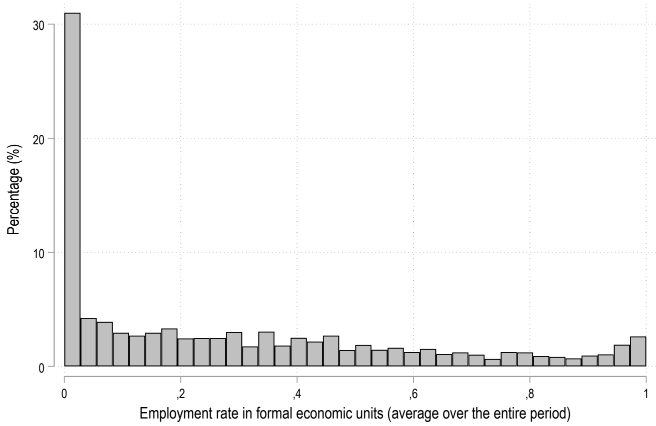
(a) Employment



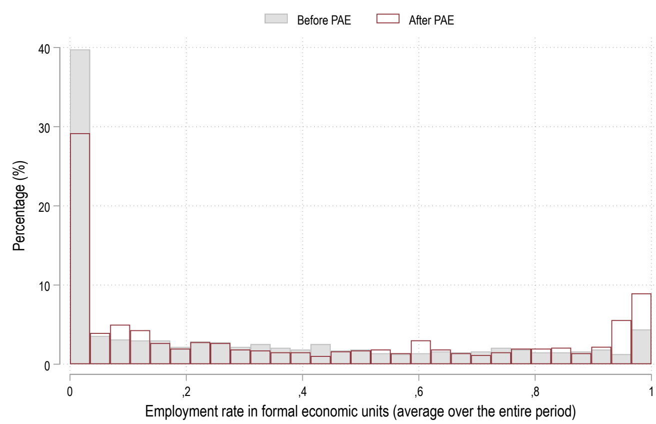
(b) Beneficiaries



(c) Employment on formal economic units

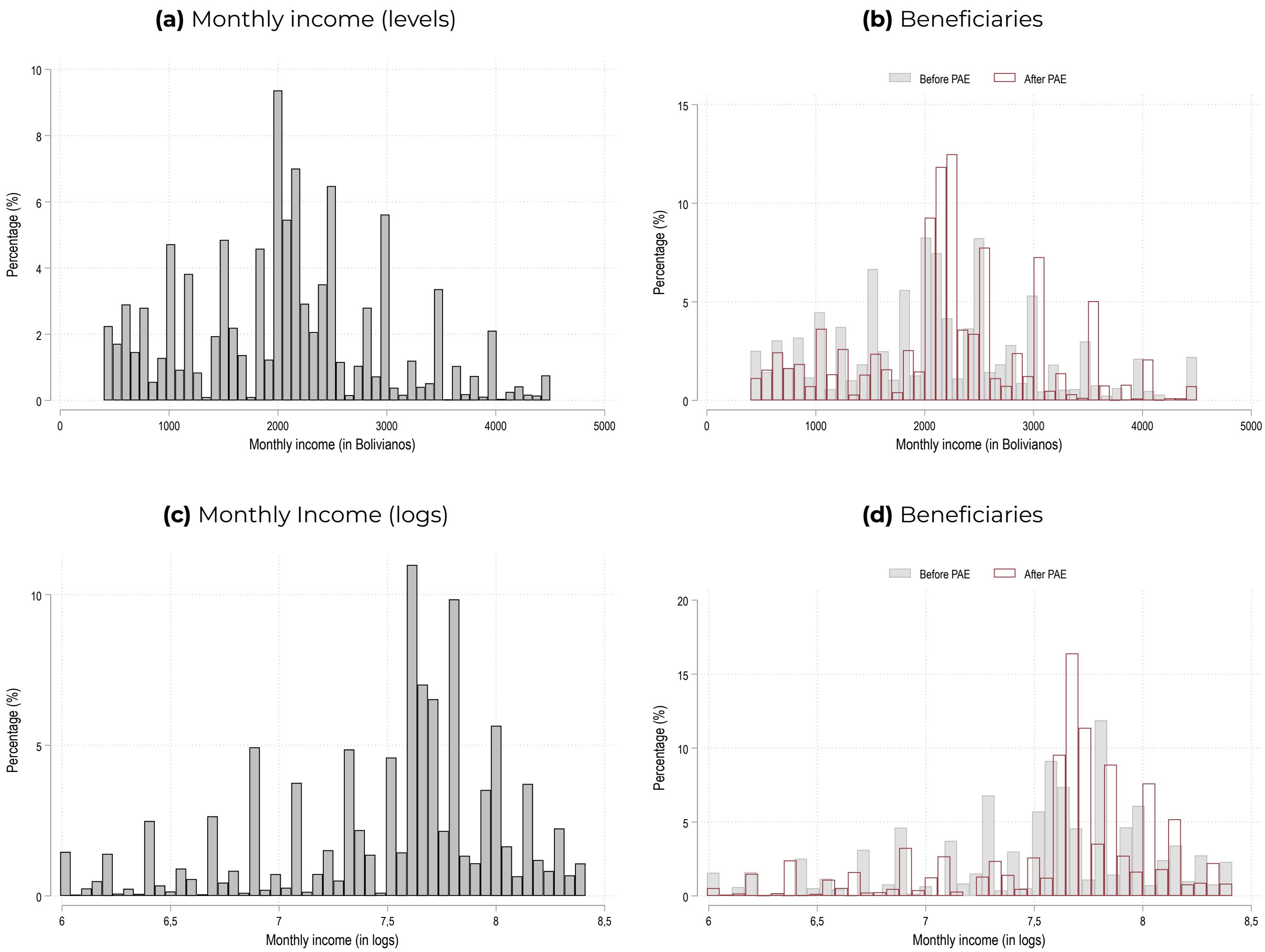


(d) Beneficiaries



Notes: The histograms presented depict the levels of employment and employment in formal economic units, capturing two different perspectives. Figures (a and c) show the percentage of observations that fall within a certain range for the entire observed sample. On the other hand, Figure (b and d) focuses specifically on those individuals who have been beneficiaries of PAE II at some point in time. The figures consider the cumulative averages over the entire period analyzed.

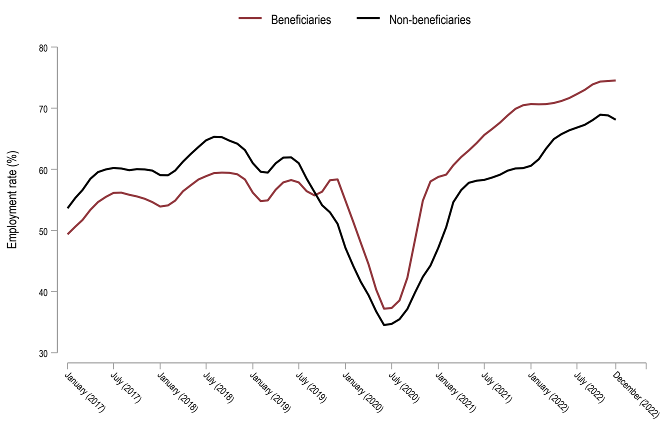
Figure A2. Statistical distributions of income variables



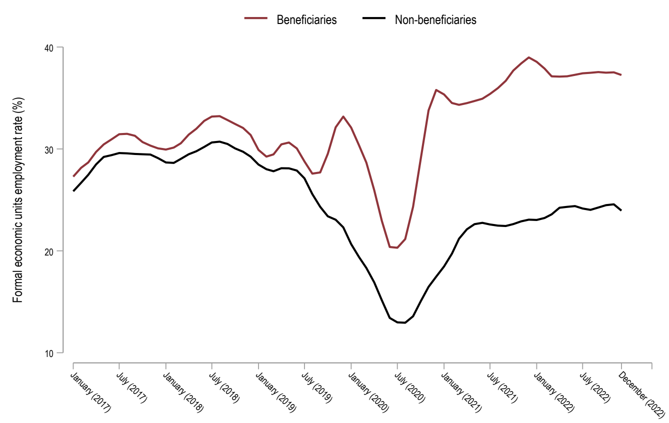
NOTES: The histograms presented depict income levels, capturing two different perspectives. Figures (a and c) show the percentage of observations that fall within a certain range for the entire observed sample. On the other hand, Figure (b and d) focuses specifically on those individuals who have been beneficiaries of PAE II at some point in time. The figures consider each observation individually, taking into account both the identification of each individual and the specific period.

Figure A3. Time evolution of outcome variables (whole sample)

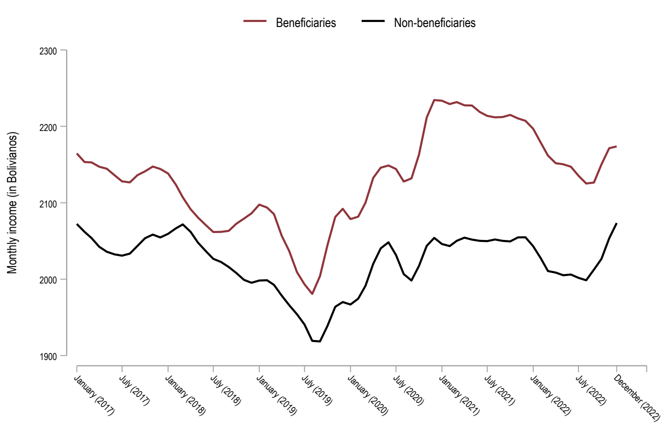
(a) Employment



(b) Employment on formal economic units



(c) Monthly income (bolivianos)

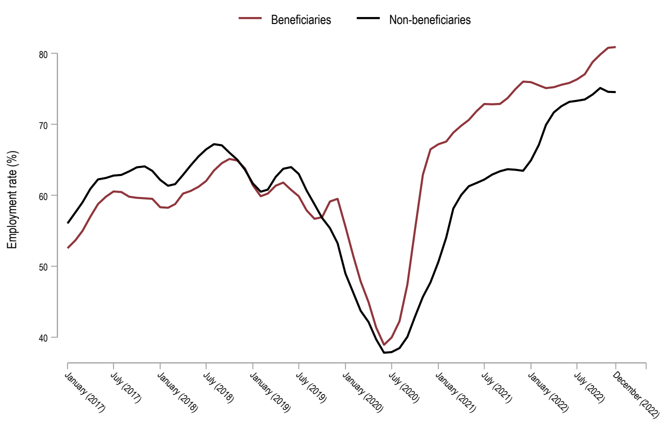


(d) Monthly Income (logs)

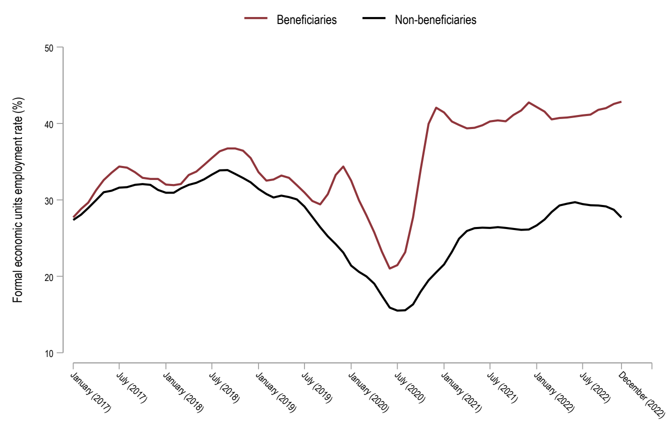
NOTES: Figures (a), (b), (c) and (d) illustrate the temporal evolution of the outcome variables for EAP II beneficiaries (treated) and non-beneficiaries (controls), using a 3-month moving average. Each point represents the average of the variable in periods t, $t-1$ and $t-2$, except for the first two periods.

Figure A4. Time evolution of outcome variables (men)

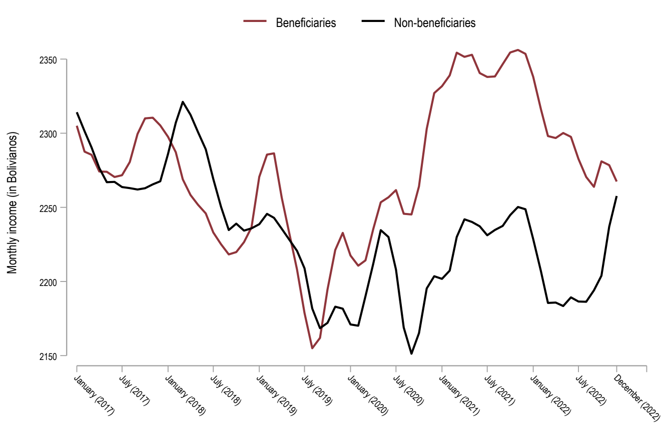
(a) Empleo



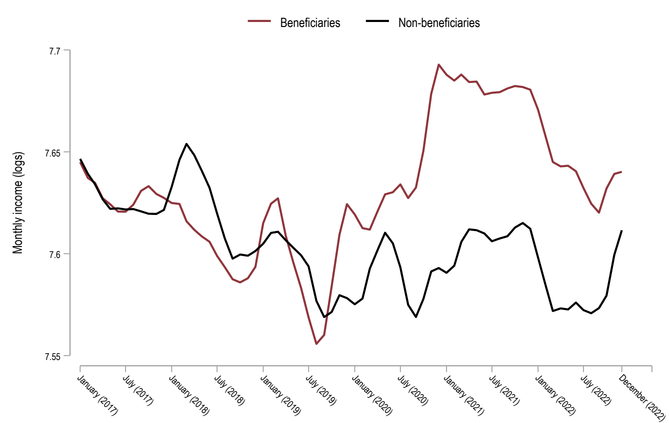
(b) Employment in formal economic units



(c) Monthly income (bolivianos)



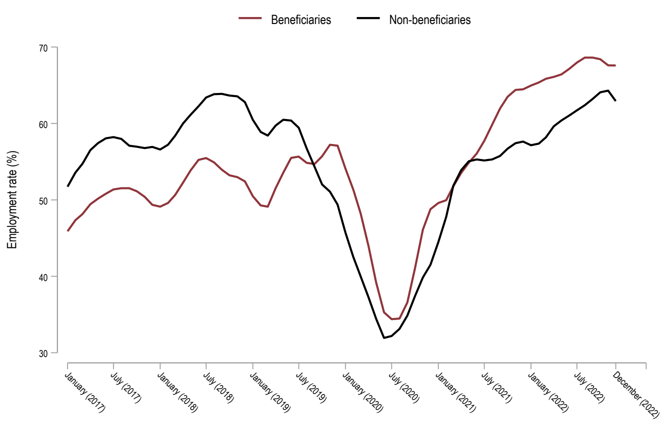
(d) Monthly Income (logs)



NOTES: Figures (a), (b), (c) and (d) illustrate the temporal evolution of the outcome variables for PAE II beneficiaries (treated) and non-beneficiaries (controls), using a 3-month moving average. Each point represents the average of the variable in periods t, $t-1$ and $t-2$, except for the first two periods.

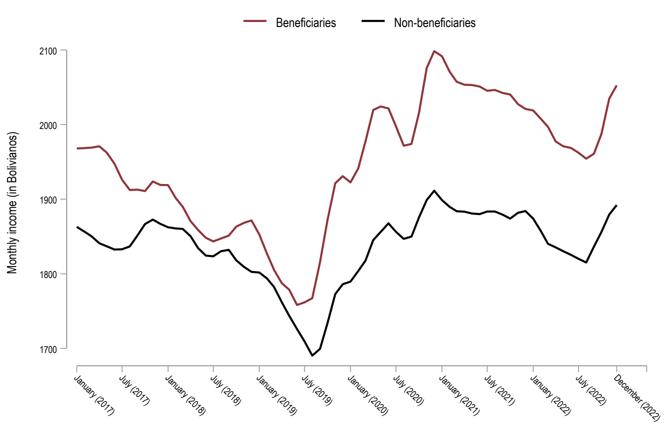
Figure A5. Time evolution of outcome variables (women)

(a) Employment



(b) Employment on formal economic units

(c) Monthly income (bolivianos)

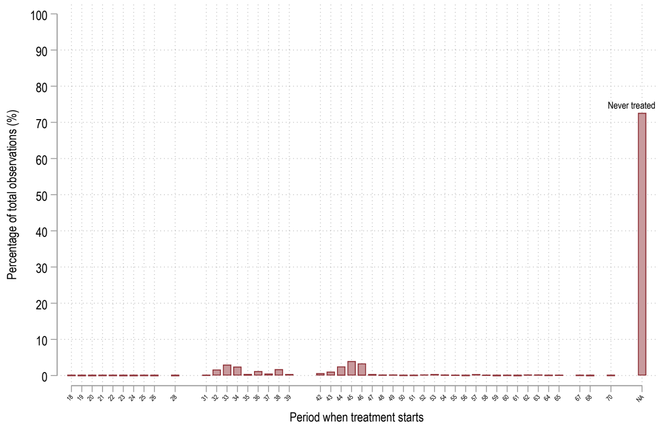


(d) Monthly income (logs)

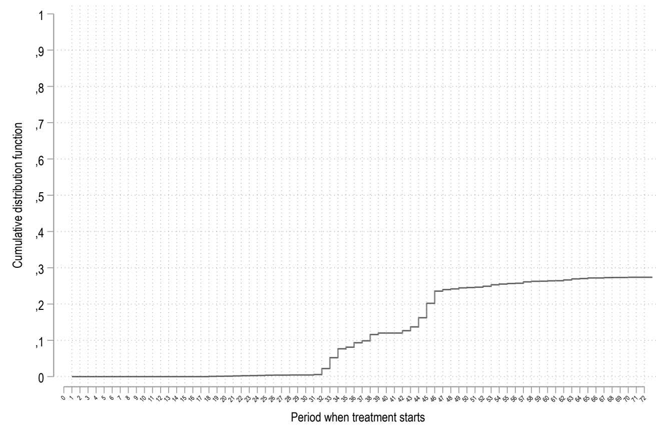
NOTES: Figures (a), (b), (c) and (d) illustrate the temporal evolution of the outcome variables for EAP II beneficiaries (treated) and non-beneficiaries (controls), using a 3-month moving average. Each point represents the average of the variable in periods t, $t-1$ and $t-2$, except for the first two periods.

Figure A6. Distribution of treated and untreated units by period in which treatment is active

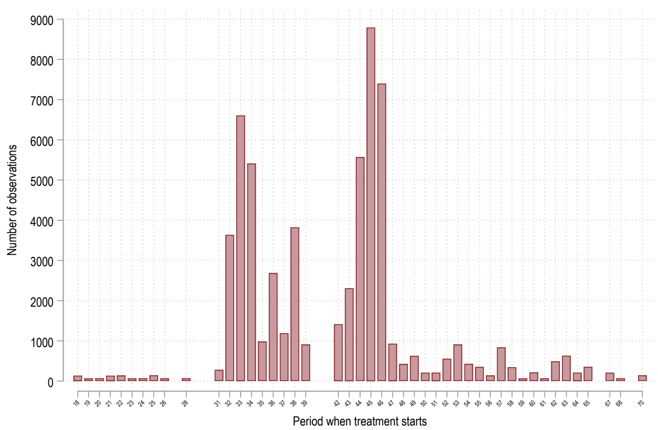
(a) Histogram: treated and untreated units



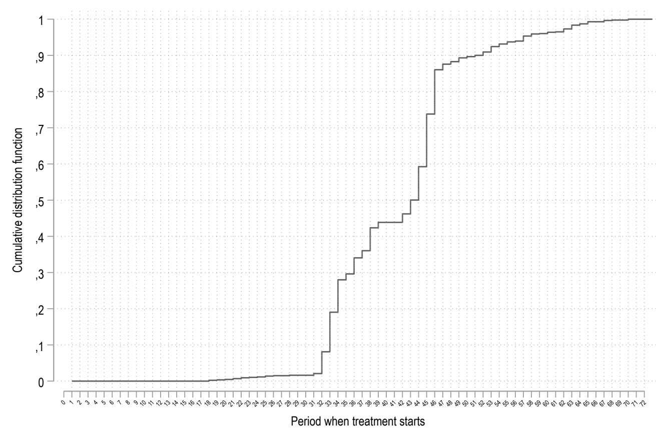
(b) CDF: treated and untreated units



(c) Histogram: treated only



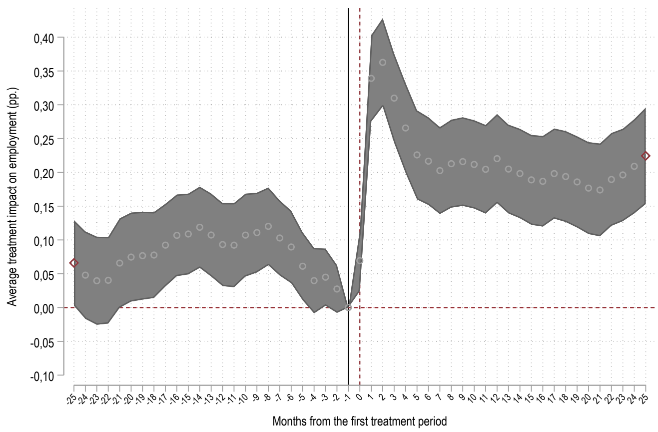
(d) CDF: treated only



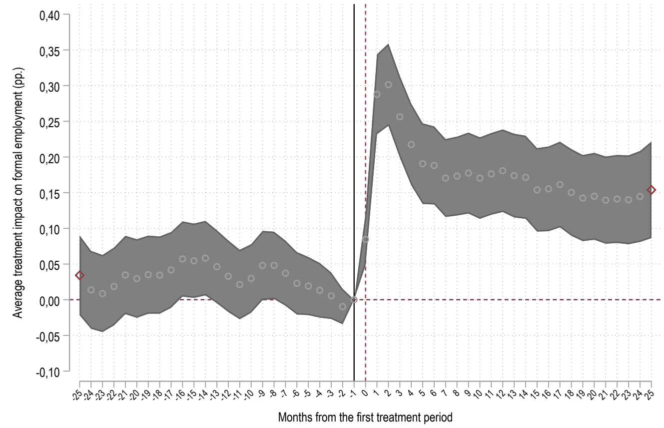
NOTES: Figures (a) and (c) present histograms of the dates on which the treatment was activated, including one observation per unit, for both treated and untreated. While Figure (a) shows the percentage that the events represent with respect to the total observations, Figure (c) uses absolute frequencies. On the other hand, Figures (b) and (d) illustrate the Cumulative Distribution Function. In this context, Figure (b) encompasses all units, treated and untreated, while Figure (d) only considers those observations belonging to the treated units. This dataset has a hybrid structure, characterized by variability in event dates among treated units and a high percentage of units that never received treatment.

Figure A7. Dynamic impact (women only): TWFE-based event study for employment, employment in formal economic units and monthly income (in levels and logs)

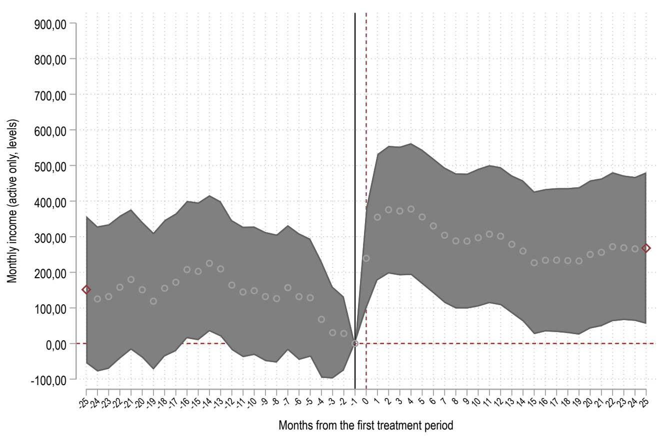
(a) Employment



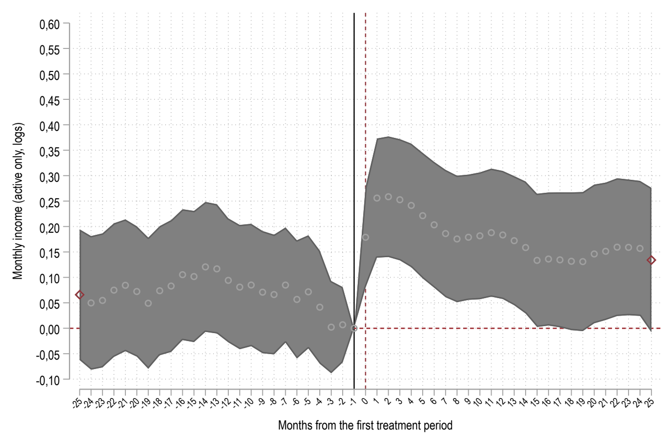
(b) Employment on formal economic units



(c) Income (in levels)



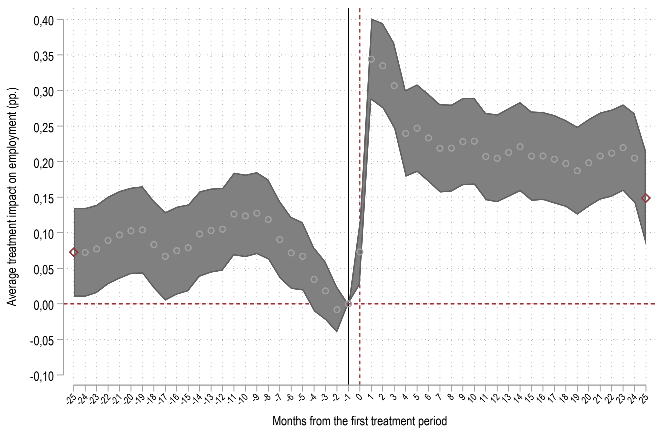
(d) Income (in logs)



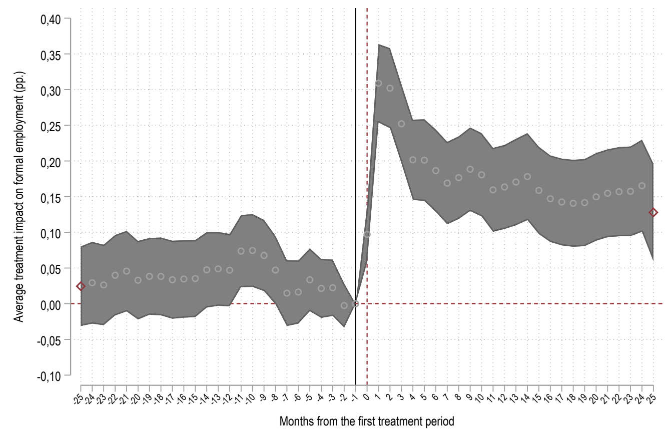
NOTES: Event study based on the estimation of the equation (2) controlling for individual characteristics and only considering women. Standard errors are clustered at the individual level. The impact is normalized with respect to the time immediately prior to treatment adoption prior to the adoption of the treatment. Periods -25 and 25 show the estimates for all periods beyond the 24-month window, thus representing long-term impacts.

Figure A8. Dynamic impact (men only): event study based on TWFE for employment, employment in formal economic units and monthly income (in levels and logs)

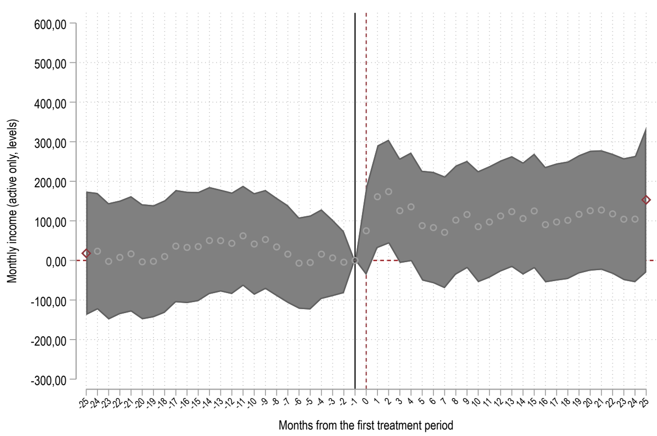
(a) Employment



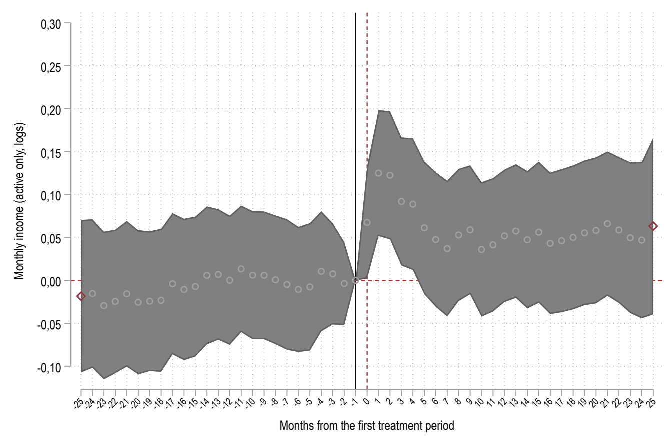
(b) Employment on formal economic units



(c) Income (in levels))



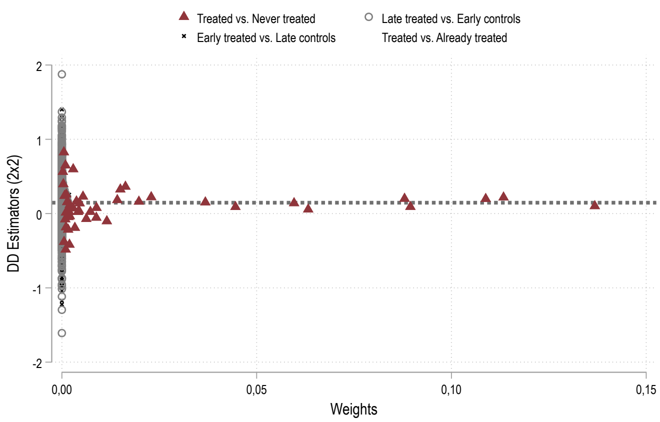
(d) Income (in logs)



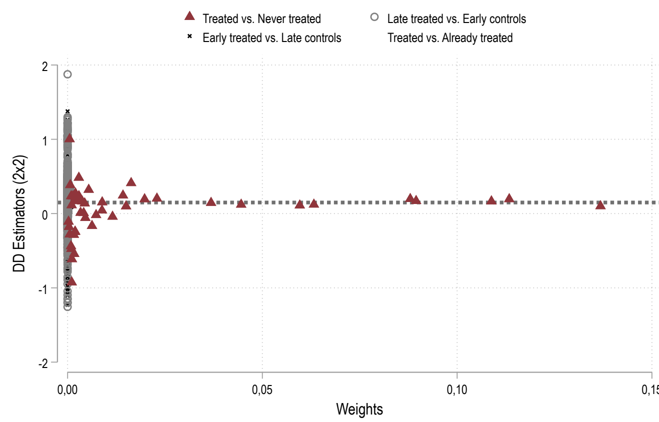
NOTES: Event study based on the estimation of the equation (2) controlling for individual characteristics and only considering men. Standard errors are clustered at the individual level. The impact is normalized with respect to the time immediately prior to treatment adoption prior to the adoption of the treatment. Periods -25 and 25 show the estimates for all periods beyond the 24-month window, thus representing long-term impacts.

Figure A9. Goodman-Bacon decomposition: employment, employment in formal economic units and monthly income (in logs)

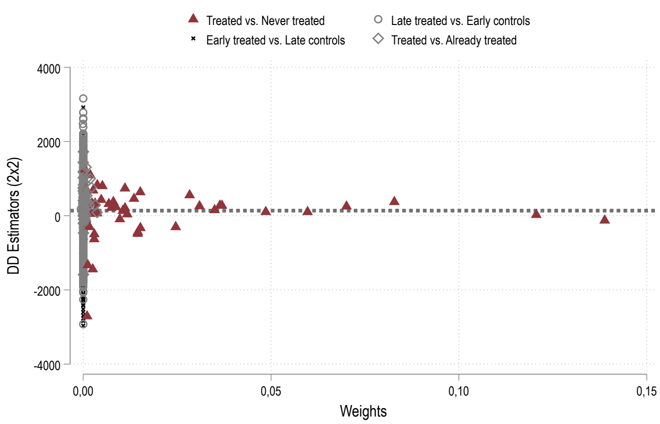
(a) Employment



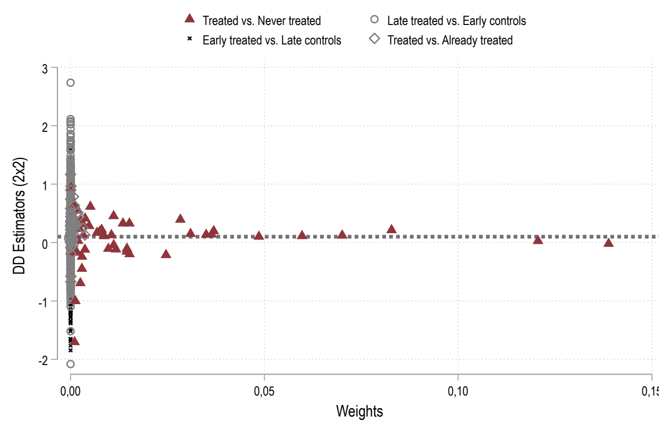
(b) Employment in economic formal units



(c) Income (in levels)

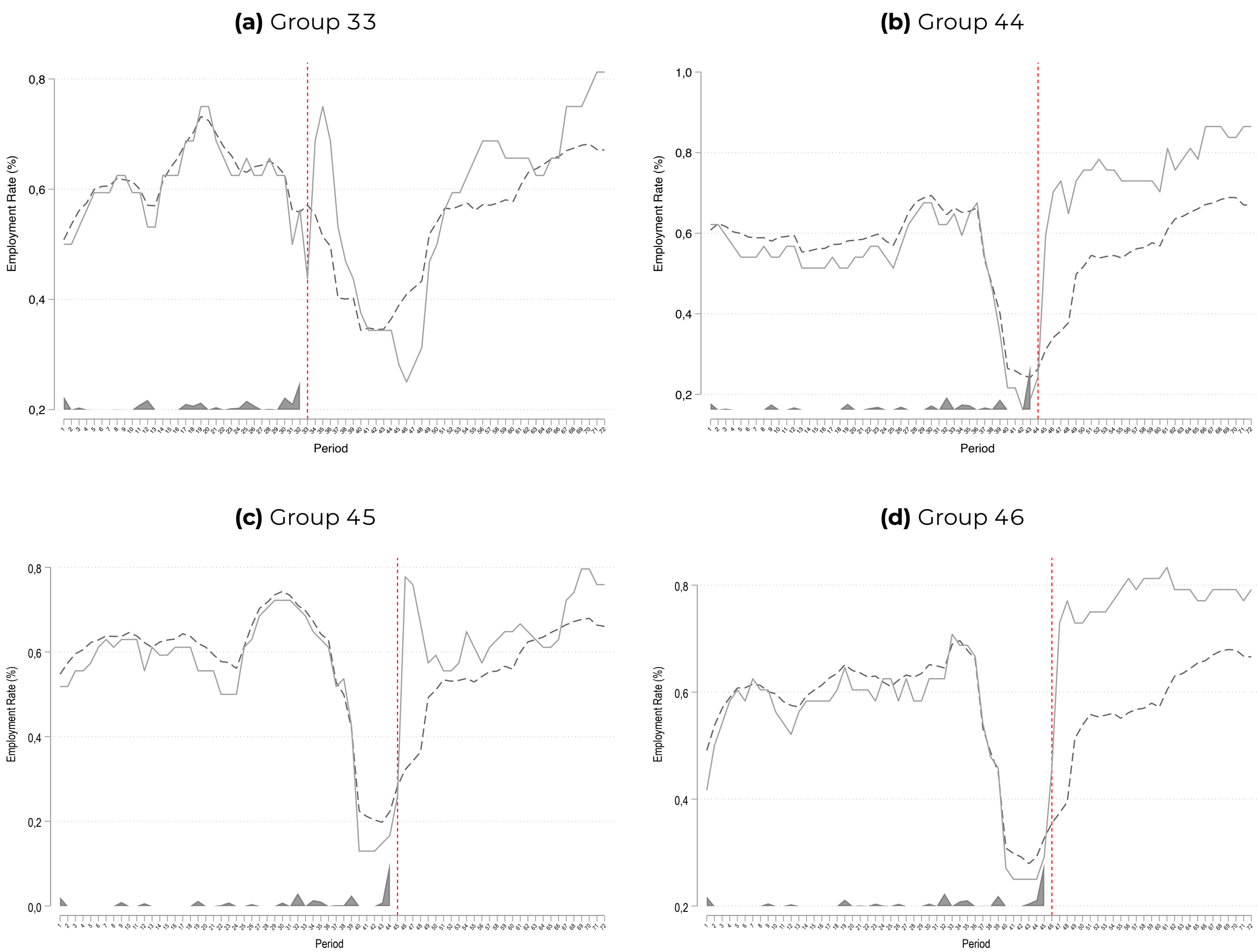


(d) Income (in logs)



NOTES: Figures document the decomposition of Goodman-Bacon (2021). The maroon △ symbols represent comparisons between treated units and pure untreated controls (never adopters of the treatment). Each triangle is an alternative estimate that depends on the timing of treatment adoption. The ⋄ symbols represent comparisons between individuals who adopted the treatment before the panel's start vs. individuals who adopted the treatment later. These individuals do not exist in employment variables, but they do in income variables, as the panel is limited by the number of strictly positive observations. The ∘ symbols represent cases where identification is based solely on temporal comparisons. The black ∘ symbols represent comparisons between units treated earlier (as treatment) and units treated later (as controls). The lighter gray ∘ symbols represent problematic comparisons between units treated later (as treatment) and units treated earlier (as controls). Note that each point in the graph considers an alternative treatment adoption period, as the treatment in our context is multiple and staggered. The overall decomposition for each of these four groups is provided in Tables A11, A12, A13 and A14.

Figure A10. Temporal Evolution of Employment According to the $SCDD$ Estimator



(a) Group 33

(b) Group 44

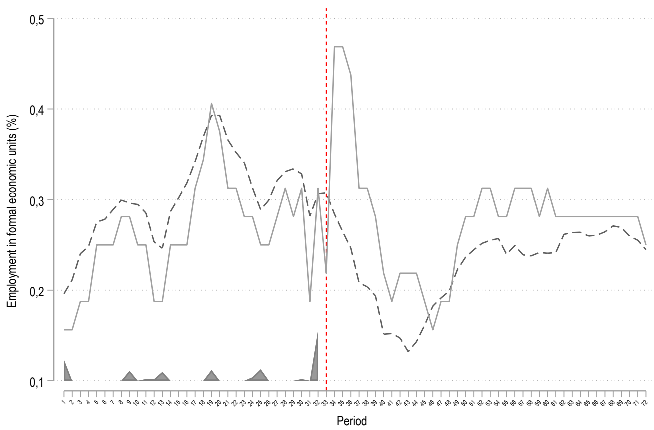
(c) Group 45

(d) Group 46

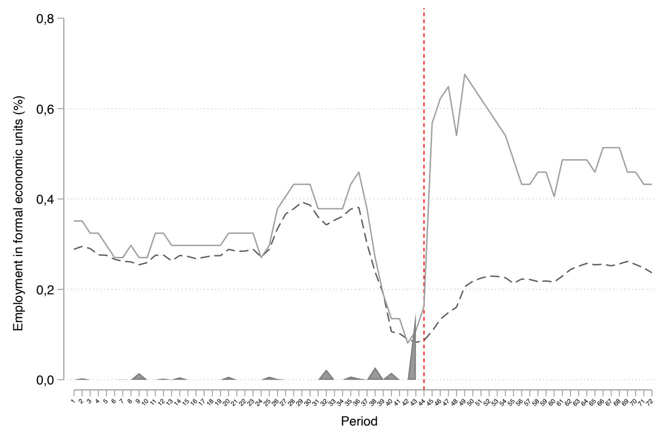
NOTES: This figure shows the employment variable trends for both the treatment group (grey line) and the control group (dashed black line) for the four events with the most observations. Each group corresponds to the following dates: September - 2020 (Group 45), October - 2020 (Group 46), August - 2020 (Group 44), and September 2019 (Group 33). Temporal weights are represented by grey triangles.

Figure A11. Temporal Evolution of Employment in Formal Economic Units According to the $SCDD$ Estimator

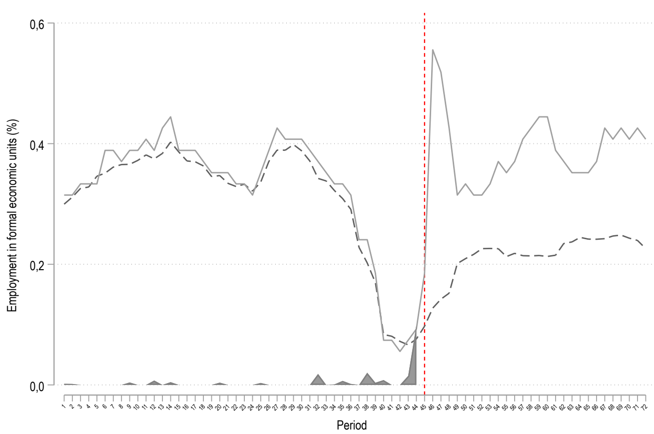
(a) Group 33



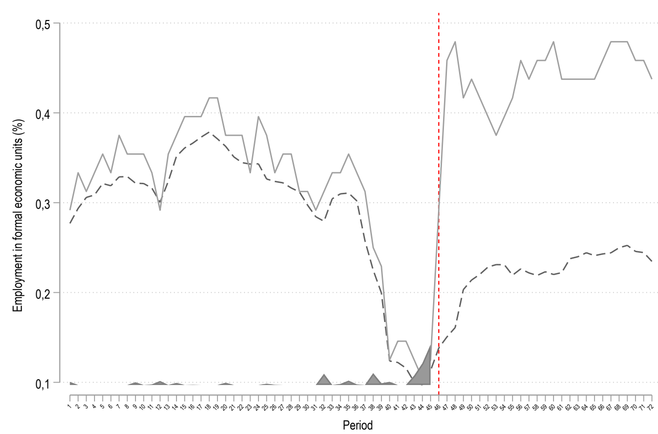
(b) Group 44



(c) Group 45



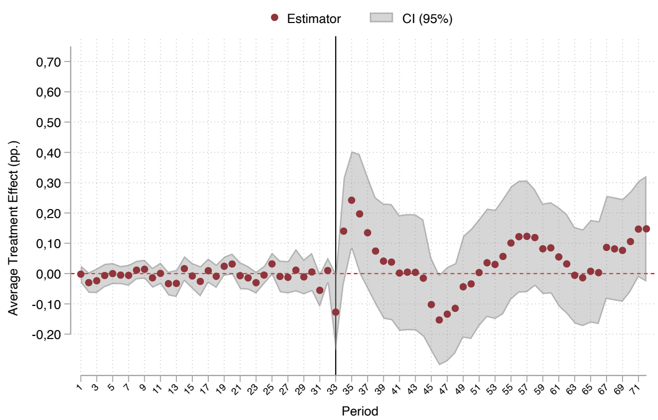
(d) Group 46



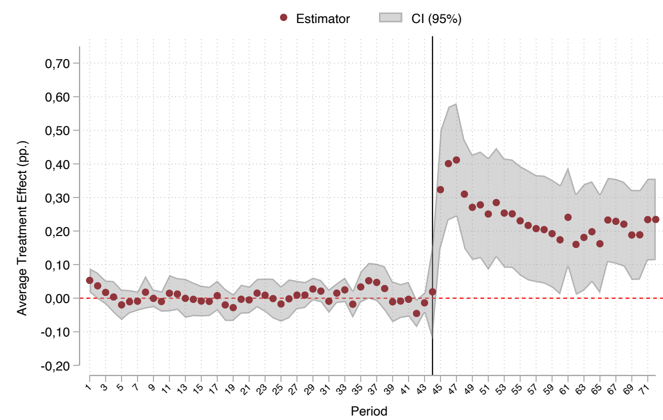
NOTES: This figure displays the trends of employment variables in formal economic units for both the treatment group (grey line) and the control group (dashed black line) for the four events with the most observations. Each group corresponds to the following dates: September - 2020 (Group 45), October - 2020 (Group 46), August - 2020 (Group 44), and September 2019 (Group 33). Temporal weights are represented by grey triangles.

Figure A12. Event Study Based on the $SCDD$ Estimator for Employment

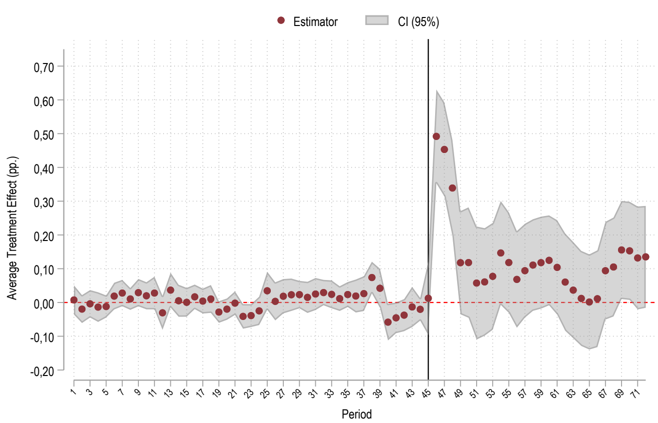
(a) Group 33



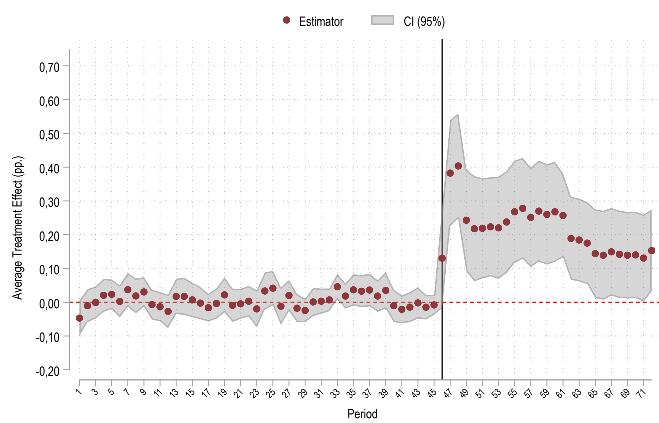
(b) Group 44



(c) Group 45



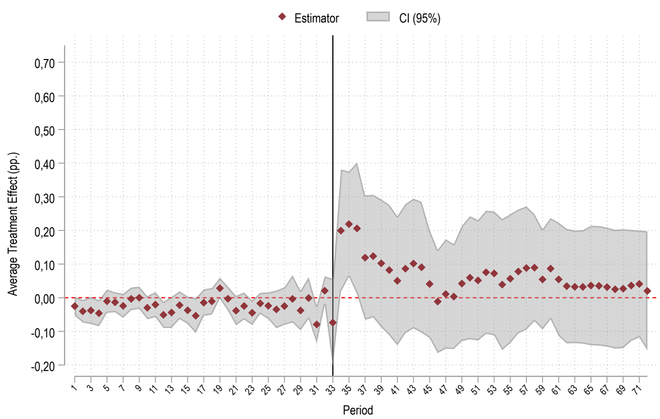
(d) Group 46



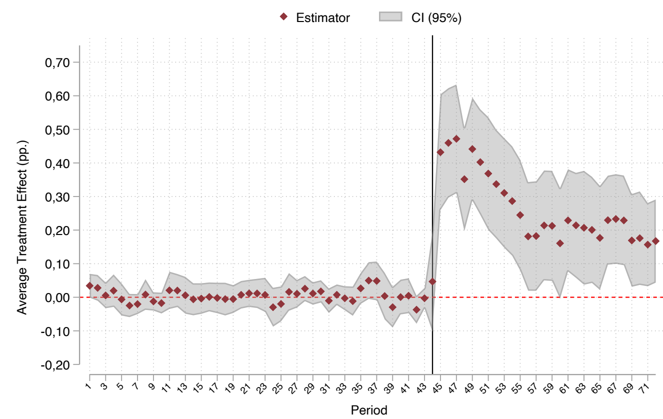
NOTES: This figure presents a typical event study, but based on the synthetic control estimator. It allows visualization of how the treatment impact evolves over time and how the differences between the treated and control groups are prior to the treatment adoption. Confidence intervals are calculated via bootstrapping considering 1,000 repetitions.

Figure A13. Event Study Based on the $SCDD$ Estimator for Employment in Formal Economic Units

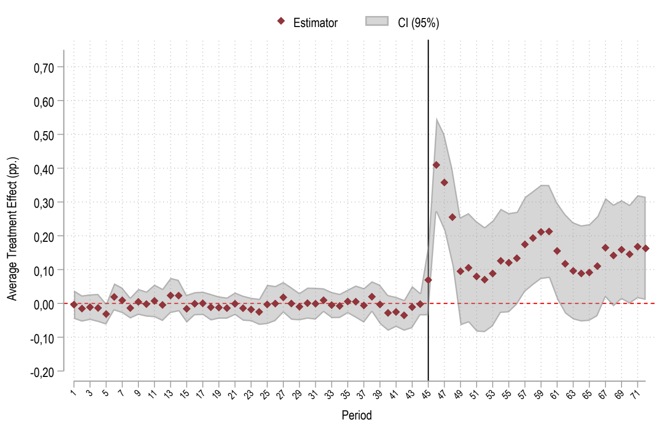
(a) Group 33



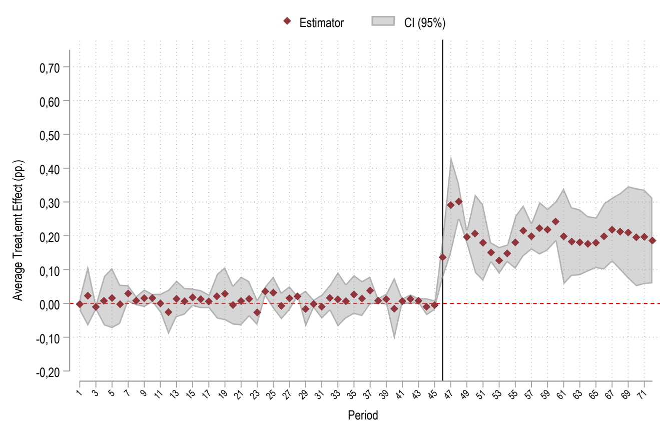
(b) Group 44



(c) Group 45



(d) Group 46



NOTES: This figure presents a typical event study, but based on the synthetic control estimator. It enables visualization of how the treatment impact evolves over time and how the differences between the treated and control groups exist prior to the treatment adoption. Confidence intervals are calculated via bootstrapping, considering 1,000 repetitions.

B Appendix: Tables

Table A1. Descriptive Statistics - Entire Sample

	Average	Std. Dev	Median	Obs.
Outcome Variables				
Employed == 1	0.57	0.49	1.00	220,697
Formal employment == 1 (having TIN)	0.27	0.44	0.00	220,697
Labor income (Bolivianos)	1,031.42	1,211.68	400.00	220,697
Labor income (active only, Bolivianos)	2,054.41	907.02	2,060.00	110,801
Labor income (logarithms)	7.51	0.53	7.63	110,801
Characteristics				
Head of household == 1	0.38	0.48	0.00	220,697
Female == 1	0.54	0.50	1.00	220,697
Age (years)	30.30	8.35	28.00	220,697
Married/living together == 1	0.31	0.46	0.00	220,697
Speaks indigenous language == 1	0.12	0.32	0.00	220,697

NOTES: Descriptive statistics for all units. There are a total of 3,140 individuals, 72 periods, and 220,697 observations. Of these, 865 are treated units (60,649 observations) and 1,029 are untreated units (160,048 observations).

Table A2. Descriptive Statistics - Treatment Group

	Average	**Std. Dev**	**Median**	**Obs.**
Outcome Variables				
Employed == 1	0.59	0.49	1.00	60,649
Formal employment == 1 (having TIN)	0.32	0.47	0.00	60,649
Labor income (Bolivianos)	1,118.29	1,247.20	600.00	60,649
Labor income (active only, Bolivianos)	2,135.83	893.07	2,142.00	31,755
Labor income (logarithms)	7.56	0.51	7.67	31,755
Characteristics				
Head of household == 1	0.40	0.49	0.00	60,649
Female == 1	0.48	0.50	0.00	60,649
Age (years)	29.45	7.88	27.00	60,649
Married/living together == 1	0.29	0.45	0.00	60,649
Speaks indigenous language == 1	0.10	0.31	0.00	60,649

NOTES: Descriptive statistics for the treated units. There are a total of 865 treated units (60,649 observations).

Table A3. Descriptive Statistics - Control Group

	Average	**Std. Dev**	**Median**	**Obs.**
Outcome Variables				
Employed == 1	0.57	0.50	1.00	160,048
Formal employment == 1 (having TIN)	0.24	0.43	0.00	160,048
Labor income (Bolivianos)	998.50	1,196.30	0.00	160,048
Labor income (active only, Bolivianos)	2,021.70	910.53	2,000.00	79,046
Labor income (logarithms)	7.49	0.54	7.60	79,046
Characteristics				
Head of household == 1	0.37	0.48	0.00	160,048
Female == 1	0.56	0.50	1.00	160,048
Age (years)	30.62	8.49	28.00	160,048
Married/living together == 1	0.32	0.47	0.00	160,048
Speaks indigenous language == 1	0.12	0.33	0.00	160,048

NOTES: Descriptive statistics for the untreated units. There are a total of 1,029 untreated units (160,048 observations).

Table A4. Descriptive Statistics: Treated and Control Units (Pre-treatment)

	Controls	Treated	Difference
Employed == 1	0.57	0.53	0.04***
Formal employment == 1 (having TIN)	0.24	0.28	-0.04***
Labor income (Bolivianos)	998.50	952.85	45.65***
Labor income (active only, Bolivianos)	2,021.70	2,080.43	-58.73***
Labor income (logarithms)	7.49	7.52	-0.03***
Head of household == 1	0.37	0.36	0.01***
Female == 1	0.56	0.48	0.08***
Age (years)	30.62	28.46	2.16***
Married/living together == 1	0.32	0.27	0.05***
Speaks indigenous language == 1	0.12	0.11	0.01***
Joint Significance Test	0.000		

Table A5. DID Estimates: Labor Market Outcomes After Program Participation (No Controls)

	Employment	Employment (Formal)	Income (Levels)	Income (Logs)
Treatment == 1	0.146***	0.149***	128.563***	0.095***
	(0.016)	(0.017)	(44.933)	(0.026)
Observations	220,697	220,697	110,791	110,791
Individual F.E.	Yes	Yes	Yes	Yes
Temporal F.E.	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Avg. Treated	0.35	0.17	1,907	7.42

NOTES: This table explores the effect of participation in the PAE II on incomes (active only) both in levels and logarithms. It also shows the impact of the program on employment and formal economic unit employment. All estimates include individual and monthly fixed effects. Standard errors are clustered at the individual level and are presented in parentheses. For each outcome variable, the average of that variable for the treated group immediately before the treatment started is presented. * p<0.10; ** p<0.05; *** p<0.01.

Table A6. DID Estimates: Labor Market Outcomes After Program Participation (With Controls)

	Employment	Employment (Formal)	Income (Levels)	Income (Logs)
Treatment == 1	0.143***	0.146***	117.486***	0.087***
	(0.016)	(0.017)	(45.236)	(0.026)
Observations	220,697	220,697	110,791	110,791
Individual F.E.	Yes	Yes	Yes	Yes
Temporal F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Avg. Treated	0.35	0.17	1,907	7.42

NOTES: This table explores the effect of participation in the PAE II on incomes (active only) both in levels and logarithms. It also shows the impact of the program on employment and formal economic unit employment. All estimates include individual and monthly fixed effects. Standard errors are clustered at the individual level and are presented in parentheses. For each outcome variable, the average of that variable for the treated group immediately before the treatment started is presented. * p<0.10; ** p<0.05; *** p<0.01.

Table A7. TWFE Estimates: Labor Market Outcomes after Participating in the Program (without Controls, by Gender)

	Employment		Formal Employment	
	Women	Men	Women	Men
Treatment == 1	0.148***	0.140***	0.149***	0.143***
	(0.024)	(0.021)	(0.024)	(0.024)
Observations	118,537	102,160	118,537	102,160
Individual Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Treated Group Mean	0.32	0.38	0.14	0.30

NOTES: This table explores the effect of participating in PAE II on income (only active) both in levels and logarithms. It also shows the impact of the program on employment and employment in formal economic units. All estimates include monthly and individual fixed effects. Standard errors are clustered at the individual level and are in parentheses. For each outcome variable, the mean for the treated group is presented in the period immediately prior to the start of the treatment. * p<0.10; ** p<0.05; *** p<0.01.

Table A8. DID Estimates by Gender: Labor Market Outcomes After Program Participation (With Controls, by Gender)

	Employment		**Formal**	
	Female	Male	Female	Male
Treatment == 1	0.146***	0.137***	0.144***	0.142***
	(0.024)	(0.021)	(0.024)	(0.024)
Observations	118,537	102,160	118,537	102,160
Individual Fixed Effects	Yes	Yes	Yes	Yes
Temporal Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Avg. Treated	0.32	0.38	0.14	0.19

NOTES: This table explores the effect of participation in the PAE II on incomes (active only) both in levels and logarithms. It also shows the impact of the program on employment and employment in formal economic units. All estimates include individual and monthly fixed effects. Standard errors are clustered at the individual level and are presented in parentheses. For each outcome variable, the average of that variable for the treated group immediately before the treatment started is presented. * p<0.10; ** p<0.05; *** p<0.01.

Table A9. TWFE Estimates: Labor Market Outcomes after Participating in the Program (without Controls, by Gender)

	Income (Levels)		**Income (Logs)**	
	Women	Men	Women	Men
Treatment == 1	155.256**	111.472*	0.117***	0.081**
	(62.339)	(63.511)	(0.039)	(0.035)
Observations	55,555	55,236	55,555	55,236
Individual Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Treated Group Mean	1.625	2.095	7.24	7.64

NOTES: This table explores the effect of participating in PAE II on income (only active) both in levels and logarithms. It also shows the impact of the program on employment and employment in formal economic units. All estimates include monthly and individual fixed effects. Standard errors are clustered at the individual level and are in parentheses. For each outcome variable, the mean for the treated group is presented in the period immediately prior to the start of the treatment. * p<0.10; ** p<0.05; *** p<0.01.

Table A10. TWFE Estimates by Gender: Labor Market Outcomes after Participating in the Program (with Controls, by Gender)

	Income (Levels)		**Income (Logs)**	
	Women	Men	Women	Men
Treatment == 1	144.028**	98.716	0.106***	0.074**
	(62.943)	(63.619)	(0.039)	(0.035)
Observations	55,555	55,236	55,555	55,236
Individual Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Treated Group Mean	1.625	2.095	7.24	7.64

NOTES: This table explores the effect of participating in PAE II on income (only active) both in levels and logarithms. It also shows the impact of the program on employment and employment in formal economic units. All estimates include monthly and individual fixed effects. Standard errors are clustered at the individual level and are in parentheses. For each outcome variable, the mean for the treated group is presented in the period immediately prior to the start of the treatment. * p<0.10; ** p<0.05; *** p<0.01.

Table A11. Goodman-Bacon Decomposition: Employment

	Weight	**DDEF Estimator**
Aggregate Average DD Estimator	.	0.146
Early Treated vs. Late Controls	0.05	0.195
Late Treated vs. Early Controls	0.03	0.185
Treated vs. Never Treated	0.93	0.143
Treated vs. Already Treated Units	.	.

Table A12. Goodman-Bacon Decomposition: Formal Employment

	Weight	**DDEF Estimator**
Aggregate Average DD Estimator	.	0.149
Early Treated vs. Late Controls	0.05	0.189
Late Treated vs. Early Controls	0.03	0.217
Treated vs. Never Treated	0.93	0.145
Treated vs. Already Treated Units	.	.

Table A13. Goodman-Bacon Decomposition: Monthly Income (Levels)

	Weight	**DDEF Estimator**
Aggregate Average DD Estimator	.	136
Early Treated vs. Late Controls	0.031	396
Late Treated vs. Early Controls	0.022	159
Treated vs. Never Treated	0.922	119
Treated vs. Already Treated Units	0.025	421

Table A14. Goodman-Bacon Decomposition: Monthly Income (Logs)

	Weight	**DDEF Estimator**
Aggregate Average DD Estimator	.	0.1
Early Treated vs. Late Controls	0.03	0.241
Late Treated vs. Early Controls	0.02	0.151
Treated vs. Never Treated	0.92	0.088
Treated vs. Already Treated Units	0.03	0.3

Table A15. Employment

	All	Women	Men
Simple Weighted Average	0.210***	0.217***	0.197***
	(0.019)	(0.027)	(0.029)
Observations	220,697	102,128	118,491

Table A16. Employment in Formal Economic Units

	All	Women	Men
Simple Weighted Average	0.179***	0.175***	0.178***
	(0.018)	(0.026)	(0.026)
Observations	220,696	102,128	118,491

Table A17. Monthly Income (levels)

	All	Women	Men
Simple Weighted Average	282,074***	350,450***	244,160***
	(70,031)	(122,954)	(84,781)
Observations	100,001	49,416	50,503

Table A18. Monthly Income (logs)

	All	Women	Men
Simple Weighted Average	0.157***	0.210***	0.128***
	(0.041)	(0.078)	(0.047)
Observations	100,001	49,416	50,503

Table A19. Impact at Different Treatment Exposure Times on Employment (pp.): 6, 12, 18, and 24 Months

	Estimators	**Standard Error**
6 months	0.25	0.022
12 months	0.22	0.023
18 months	0.19	0.023
24 months	0.20	0.024

Table A20. Impact at Different Treatment Exposure Times on Employment in Formal Economic Units (pp.): 6, 12, 18, and 24 Months

	Estimators	Standard Error
6 months	0.22	0.02
12 months	0.18	0.021
18 months	0.16	0.022
24 months	0.16	0.022

Table A21. Impact at different exposure times to treatment on incomes (levels): 6, 12, 18, and 24 months

	Estimators	Standard Error
6 months	369	69
12 months	326	82
18 months	245	87
24 months	311	95

Table A22. Impact at Different Treatment Exposure Times on Income (logs): 6, 12, 18, and 24 Months

	Estimators	Standard Error
6 months	0.22	0.04
12 months	0.18	0.047
18 months	0.13	0.053
24 months	0.16	0.058

Table A23. Trend Pre-treatment Test. Null Hypothesis (H0): All pre-treatment differences are equal to 0 (employment)

	Value
Chi2	522049
p-value	0.000

Table A24. Trend Pre-treatment Test. Null Hypothesis (H0): All pre-treatment differences are equal to 0 (formal employment)

	Value
Chi2	793629
p-value	0.000

Table A25. Trend Pre-treatment Test. Null Hypothesis (H0): All pre-treatment differences are equal to 0 (income (level))

	Value
Chi2	1730263
p-value	0.000

Table A26. Trend Pre-treatment Test. Null Hypothesis (H0): All pre-treatment differences are equal to 0 (income (logs))

	Value
Chi2	11034558
p-value	0.000

Table A27. CSDD Estimators for Employment Variables

	Employment			**Formal Employment**		
	All	Women	Men	All	Women	Men
Treatment == 1	0.139***	0.157***	0.121***	0.168***	0.174***	0.158***
	(0.032)	(0.039)	(0.041)	(0.023)	(0.039)	(0.033)
Observations	91,872	48,456	43,344	91,872	48,456	43,344

NOTES: The results are based on the estimator by Arkhangelsky et al. (2021) and measure the impact of the program on employment and employment in formal economic units. Standard errors are calculated via *bootstrap* considering 50 repetitions. * p<0.10; ** p<0.05; *** p<0.01.

Table A28. CSDD Estimators for Income Variables

	Income (Levels)			**Income (Logs)**		
	All	Women	Men	All	Women	Men
Treatment == 1	191.019	-6.166	31.616	0.341	-0.050	0.280
	(656.417)	(802.922)	(1140.569)	(0.369)	(0.663)	(0.491)
Observations	1,890	840	1,050	1,890	840	1,050

NOTES: The results are based on the estimator by Arkhangelsky et al. (2021) and measure the impact of the program on income levels and logs only for active individuals. Standard errors are calculated via *bootstrap* considering 1000 repetitions. * p<0.10; ** p<0.05; *** p<0.01.

Table A29. TWFE Estimates by Gender: Labor Market Outcomes after Participating in PAE I (without Controls, by Gender)

	Employment			**Formal Employment**		
	All	Women	Men	All	Women	Men
PAE I Beneficiary	0.112***	0.103***	0.122***	0.063***	0.057***	0.068***
	(0.012)	(0.016)	(0.018)	(0.016)	(0.022)	(0.023)
Observations	29,770	17,152	12,618	9,273	4,913	4,360
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Treated Group Mean	0.28	0.26	0.31	0.09	0.08	0.09

NOTES: This table explores the effect of participating in PAE I on employment and employment in formal economic units. All estimates include monthly and individual fixed effects. Standard errors are clustered at the individual level and are in parentheses. For each outcome variable, the mean for the treated group is presented in the period immediately prior to the start of the treatment. p<0.10; ** p<0.05; *** p<0.01.

Table A30. TWFE Estimates by Gender: Labor Market Outcomes after Participating in PAE I (without Controls, by Gender)

	Income (Levels)			Income (Logs)		
	All	Women	Men	All	Women	Men
PAE I Beneficiary	219.334*	308.958	122.535	0.024**	0.041***	0.006
	(121.079)	(210.991)	(110.147)	(0.011)	(0.015)	(0.017)
Observations	8,317	4,397	3,920	8,220	4,334	3,886
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Treated Group Mean	2,004	1,780	2,245	7.3	7.15	7.45

NOTES: This table explores the effect of participating in PAE I on income in levels and logarithms. All estimates include monthly and individual fixed effects. Standard errors are clustered at the individual level and are in parentheses. For each outcome variable, the mean for the treated group is presented in the period immediately prior to the start of the treatment. * p<0.10; ** p<0.05; *** p<0.01.

Table A31. TWFE Estimates by Gender: Labor Market Outcomes after Participating in PAE I (with Controls, by Gender)

	Employment			Formal Employment		
	All	Women	Men	All	Women	Men
PAE I Beneficiary	0.112***	0.103***	0.122***	0.059***	0.053**	0.066***
	(0.012)	(0.016)	(0.018)	(0.016)	(0.022)	(0.024)
Observations	29,770	17,152	12,618	9,273	4,913	4,360
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Treated Group Mean	0.28	0.26	0.31	0.09	0.08	0.09

NOTES: This table explores the effect of participating in PAE I on employment and employment in formal economic units. All estimates include monthly and individual fixed effects. Standard errors are clustered at the individual level and are in parentheses. For each outcome variable, the mean for the treated group is presented in the period immediately prior to the start of the treatment. * p<0.10; ** p<0.05; *** p<0.01.

Table A32. TWFE Estimates by Gender: Labor Market Outcomes after Participating in PAE I (with Controls, by Gender)

	Income (Levels)			**Income (Logs)**		
	All	Women	Men	All	Women	Men
PAE I Beneficiary	219.188*	308.220	122.044	0.024**	0.041***	0.005
	(121.108)	(211.066)	(110.308)	(0.011)	(0.015)	(0.017)
Observations	8,317	4,397	3,920	8,220	4,334	3,886
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Treated Group Mean	2,004	1,780	2,245	7.3	7.15	7.45

NOTES: This table explores the effect of participating in PAE I on income in levels and logarithms. All estimates include monthly and individual fixed effects. Standard errors are clustered at the individual level and are in parentheses. For each outcome variable, the mean for the treated group is presented in the period immediately prior to the start of the treatment. * p<0.10; ** p<0.05; *** p<0.01.

Table A33. CS Estimators for Employment Variables - PAE I

	Employment			**Formal Employment**		
	All	Women	Men	All	Women	Men
PAE I Beneficiary	0.090***	0.084***	0.093***	0.059***	0.053***	0.062***
	(0.002)	(0.006)	(0.012)	(0.014)	(0.004)	(0.002)
Observations	29,770	17,152	12,618	3,138	1,490	1,648

NOTES: The results are based on the estimator by Abadie (2021) and measure the impact of the program on employment and employment in formal economic units. Standard errors are calculated via *bootstrap* considering 50 repetitions. * p<0.10; ** p<0.05; *** p<0.01.

Table A34. CS Estimators for Income Variables - PAE I

	Income (Levels)			**Income (Logs)**		
	All	Women	Men	All	Women	Men
PAE I Beneficiary	243.583***	300.106***	165.689*	0.072***	0.089**	0.042
	(2.019)	(28.863)	(84.609)	(0.022)	(0.038)	(0.037)
Observations	7,690	4,080	3,610	7,606	4,028	3,578

NOTES: The results are based on the estimator by Abadie (2021) and measure the impact of the program on income levels and logs. Standard errors are calculated via *bootstrap* considering 50 repetitions. * p<0.10; ** p<0.05; *** p<0.01.